JOURNAL

OF THE

FRANKLIN INSTITUTE.

OF THE STATE OF PENNSYLVANIA,

FOR THE PROMOTION OF THE MECHANIC ARTS.

VOL. CXII. JULY, 1881. No. 1.

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THE DIRECT MANUFACTURE OF IRON FROM ORE, PUDDLING, HEATING FURNACE AND FORGE CINDERS, SHOWING THE ECONOMICAL ADAPTATION OF THE PROCESS FOR COMMERCIAL IRON FOR OPEN HEARTH AND FOR CRUCIBLE STEEL.

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Read before the Franklin Institute, June 17, 1881.

I have the honor to read a third paper before this venerable and respected Institute upon the manufacture of iron.

It may be remembered that in the fall of 1878 I explained a method of producing wrought iron direct from ore by reducing it with carbonaceous matter and fluxes.

The mixture filled into thin sheet iron cases, and charged into reverberatory furnaces, is subjected to a gradually increasing heat for three or four hours, when it becomes changed to metal interspersed with slag, but still surrounded with the iron cases, more or less intact.

One or more of these metallic lumps, pressed into the shape of a ball, is then shingled and rolled to a bar at the same heat.

About 50 tons of iron was made by this method from ore, then piled, reheated, rolled, cut up, and melted in crucibles to steel. After a very careful test of this steel for various purposes, the stock was pronounced

by skillful manufacturers equal to the best American or Swedish iron for the finest grades of steel.

A year later I read a second paper reporting the result of further investigation. During this interval a variety of ores had been worked. They were deoxidized not only with charcoal, as the first had been, but also *with bituminous and anthracite coal.* Some of these samples reduced with mineral coal, after reheating and rolling to bars, were converted to crucible cast steel and forged to tools. The result proved that whether deoxidation was produced with charcoal, with bituminous, or with the wasted anthracite coal slack dust of our mines, a high quality of steel was always produced, the tools from which have withstood the severe test of turning chilled rolls and planing hard cast iron.

With this brief reference to former papers, which have been printed in your JOURNAL, I pass on to detail further progress. Since that period various practical operations have been conducted for weeks together at different rolling mills, in charging ore mixtures in sheet iron cases upon the hearths of reverberatory furnaces.

After having fully demonstrated that a simple and cheap combination of fluxes with ore and carbon will always produce a uniform and satisfactory quality of metal for steel purposes, my attention has been closely directed to the commercial side of the operation. My aim has been to prove that this system can take its place in economic competition with the other older methods now in use.

In the course of these experiments I discovered that sheet iron cases may be dispensed with, without prejudice to the yield and quality of the metal. This discovery is important, as it sums up a large saving in the cost of production.

I early found that complete reduction in reverberatory furnaces could not be accomplished through a thickness of three or four inches of pulverized ore mixtures in as many hours, and yet, unless the process is hastened to a much shorter time, experience has proved that the waste of fuel, metal and labor will determine its commercial uselessness.

It was to overcome this difficulty that *annular* sheet iron cases were adopted, with a thickness of not more than five inches between the outside and inside walls of the metal mixture. By this arrangement the heat was not required to travel over 2½ inches in order to penetrate throughout the mass.

Although sheet iron cases are now dispensed with, the mixture is still moulded by *preference* into cylindrical annular shapes, as the pipe form is considered best for their rapid and cheap manufacture by machinery, as drain pipe are usually made.

It is quite important to have the mixture moulded to shapes, so that by standing them on end throughout the furnace hearth a sufficient *paying* quantity can be operated upon at one heat, while at the same time there is presented large surface areas for heat penetration.

Pulverized ore mixtures, leveled over the hearths of reverberatory furnaces, have often been reduced and *balled*, and if not over two or three inches of thickness, iron is usually made, but as the mass is heated only from the upper surface, the bottom being cold, the prolonged high heat required to penetrate this non-conducting mixture causes excessive waste both of material and labor, together with a small yield of metal.

In previous papers I attempted to explain that the greatest impediment to commercial success of direct processes has been, if I may be allowed so to express it, the failure to realize *the low heat conductivity* of ore mixtures. The reducing heat has been expected to penetrate more rapidly than the nature of the material will conduct it.

The thin annular cylindrical shape overcomes this difficulty. They are made about 15 to 18 inches high, 8 inches diameter and $2\frac{3}{4}$ inches thickness, and placed throughout the hearth so as almost to touch each other. Thus arranged, the furnace will produce as much wrought iron from ore at a heat of no longer duration than is required to produce the same quantity from pig iron.

It is not necessary to be confined to the annular cylindrical shape, although, perhaps, it may ultimately be found the best. Any form that may be easily handled—that will have sufficient base to afford a firm support in the furnace—and that will be thin enough to present large surfaces for heat penetration, will answer the purpose. They may be moulded to form on end, the shape of the letter C, a cylinder almost closed. They may be S shape or D shape on end. Many forms may suggest themselves, as easily moulded by machinery, which will be thin, and yet present large surface areas for penetration.

Formerly, in making my experiments I used reverberatory furnaces with *sand bottoms*, similar to those for heating iron piles. In one respect these bottoms are favorable, for the silica becomes quite hot, and gives off its heat to the metal mixture placed upon it, but further

experience has proved that the alkali mingled with the ore to separate impurities, after a very few operations will partially dissolve the silica bottom. The metal will then mingle with it, and much of it waste away as a silicate of iron. For this reason silica bottoms have been discarded for the ordinary puddling furnace *cinder bottoms*, and these are found in every way satisfactory. They keep the metal clean, and are themselves uninjured by use.

In preparing the mixture several points are necessary to be observed, and, although they are subsidiary to the separation of impurities, are still very important, if not essential, to the complete success of the system.

The mixture must be so compounded as, when moulded, to withstand the shock of rough handling without breaking in transportation to the furnace.

It must also be of such consistency as to preserve its original moulded shape, while in the furnace, until thoroughly penetrated by heat. If it settles down into a mass of uniform thickness over the hearth, before it is penetrated by a reducing heat, the cost and yield will render the process unprofitable.

Both of these conditions are attained by a judicious mixture of lime and clay with pulverized ore and carbon, when a preponderance of silica is combined with the ore. Magnesian lime is preferred. With aluminous ores the clay may be largely dispensed with. These substances must be varied in proportion with the analysis of the ore, but they may always be so combined as to make a firm compact mass at low cost. Salt and manganese have been used sometimes in addition to lime and clay to aid in separating phosphorus and other impurities, but lime and clay are the main dependence.

There is one exceedingly important office of this cheap alkali mixture to which is due almost entirely the commercial economy of the process. It must form a "*non-flowing slag.*" Not simply a flux to dissolve impurities, but a combination which fills a range of usefulness very much wider than that of any ordinary flux.

As the moulded masses are spread over the hearth with spaces between them, they present not only larger surface areas for *deoxidation*, but also present shapes that would be very favorable for *re-oxidation* by the furnace gases, unless means can be provided to prevent it. While it is essential to drive the heat high from the beginning, in order to bring about quick and economical reduction to metal, it is

equally as essential that the material shall at the same time be covered and protected from the waste of high heat, which is always very oxidizing. This is one of the requirements of this "*non-flowing*" slag. By entering the pores or spaces or cells of the ore, which have been vacated by the oxygen as it passes out and forms with carbon, carbonic oxide gas, each little atom or filament of metal is immediately sealed and varnished by this glassy coating, so that the delicate particles of new-made iron are in this *manner effectually saved from re-oxidation and destruction*.

An *ordinary* flux would flow from the metal on to the bottom of the furnace, carrying, it is true, a portion of the impurities with it, but leaving the new spongy iron to melt into a protoxide, like snow melts before a south wind. Hence the importance of having this slag non-flowing, as well as fluxing, which is so very easily accomplished.

There is still another result accomplished by the proper mixture of these cheap alkali material with ore and carbon. *Being highly basic, it dephosphorizes and desulphurizes as well as desiliconizes.* This same combination, which may be depended upon to form a firm mass both out of and in the furnace, and that protects the ore from oxidizing influences by furnace gases, may be also effectually relied upon to break the affinity of iron both for phosphorus and sulphur as well as for titanic acid. A large class of rich ores may be thus utilized which are now useless, because they contain so large a percentage of these impurities. These deleterious substances are separated either by volatilization or pass off with the slag as it is expelled by the squeezer and rolls.

Still I am not yet through with extolling the virtues of this "*non-flowing slag.*" Important as its influence is for the purposes here before stated, it also serves to secure a great economy of fuel. Metallurgists and chemists agree that ore should be brought to wrought iron bars with the consumption of half a ton of coal to the ton of iron. Notwithstanding these nice theoretical estimates, the best part of three tons of coal is required to a ton of bars. This is one of those marked cases where theory and practice widely differ, in spite of every effort to correct it. Some very powerful, but perhaps not very well understood, cause must lie at the bottom of this extraordinary waste in converting ores to metal. Allow me to venture the query whether it may not be produced by *nitrogen*. The chemist will create intense

combustion in a jar of pure oxygen, but let him mingle with it a large volume of nitrogen, and combustion will be arrested.

Until lately nitrogen has been considered rather a neutral substance in iron making, being neither favorable nor injurious to the metal. Now with every 1000 pounds of air thrown into the furnace, only 230 pounds is oxygen, while of the remainder more than *700 pounds is nitrogen.* The oxygen forms not one-fourth of the total blast, and yet it is only upon the oxygen we place dependence to secure combustion. A gas which forms nearly three-fourths of the entire volume contained in the furnace, if it is not combustible itself, most certainly must retard the ignition of the other combustible gases.

May it not be possible that these imprisoned gases in the molecules of metal are the prime cause of deteriorating its quality, so that puddled pig iron will so rarely produce high grades of crucible steel? Dr. Müller, of Brandenburg, has proved by a simple and ingenious method that hydrogen and nitrogen are actually contained in very considerable quantities in iron. He practically determined that in some cases these gases formed a volume of about fifty per cent. of that of the drilled hole from which the test was made, and these experiments have since been verified by others.

In the process under consideration *there are no* gases forced by the *pressure of the blast through the metal as in the blast furnace.* As the carbonic oxide is generated by the heated mixture, by its own pressure it finds its way to the surface of the moulds. The door is open for the exit of gases, but is closed and sealed against the inroad of injurious volatile furnace impurities. The blast pressure is not sufficient to counterbalance the outward pressure of carbonic oxide, and hence gases are not introduced into the recesses of the metal to its injury, as pig iron is contaminated.

Whether the foregoing is or is not a correct solution of the problem, it is very certain that iron produced by this system is invariably of a quality that may be relied upon for the finest grades of fine crucible or open-hearth steel.

In all previous direct processes it has been considered essential to keep the furnace in a red smoky atmosphere of carbonic oxide, as the best condition for rapid reduction. This is a condition, it may be remarked, where about one-half of the fuel passes unconsumed out of the stack, and the other half is not allowed to generate more than half the heat it is capable of, if sufficiently supplied with air.

In common with others, I formerly deemed this most wasteful combustion of the fuel to carbonic oxide an essential means for the best reduction. Although I had discovered that commercial economy necessitated the application of a *high furnace heat at once*, in order to penetrate to the interior and rapidly reduce to metal, yet I was unable to apply this high heat because of its waste by a re-oxidation of the iron. After I found that a non-flowing slag could be made to remain and protect the metal, I was able to push the heat high on introducing the charge; and thus, by more economically burning the fuel to carbonic acid, bring the ore to metal very rapidly in less than two hours. I am aware that the treatment of an ordinary puddle ball to such prolonged exposure to furnace blast would oxidize and waste it largely to cinder, but it must be remembered that this combination is not that of an ordinary puddle ball. It is iron ore mingled with carbon to deoxidize it, and protected at every point by a glazing slag which prevents re-oxidation.

Saturated, as the mixture becomes, with a high heat, as fast as it can be conducted through it, the gases are observed to begin to work immediately. Every square inch of surface of these moulded masses is covered with a flame of carbonic oxide which is quickly transformed to carbonic acid upon meeting the heated furnace gases. Thus combustion is intensified by the heat evolved from the moulds themselves, throughout the entire furnace hearth. The heat given off from one mould impinges upon the mould adjoining, while the furnace walls and roof receive and reflect back the radiated heat. Every little particle of ore gives off *pure oxygen*—not oxygen diluted with three-quarters nitrogen to retard perfect combustion, as in the blast furnace—but as pure oxygen as it is possible for the chemist to generate in his laboratory. This oxygen immediately finds an atom of carbon close to it with which it assimilates and forms carbonic oxide. Thus existing gaseous impurities are removed, while other gases injurious to the iron are not *insidiously introduced with a strong blast pressure* as they are in pig iron. The practical result follows, as has been before stated, the iron is better for fine steel than puddled pig iron.

It is no longer necessary to waste fuel, ore and labor by maintaining in the furnace an atmosphere of carbonic oxide, which so needlessly prolongs the operation. The moulded mixture, upon the application of high heat, is itself a flame at all points, like dry wood

would be under similar circumstances. They produce, of *themselves*, a high heat, and thus economize fuel from the fire-grates.

Indeed, so effectual is the heat generated at the surface of the moulded mixture that I am accustomed to lessen the supply of fuel at the stoking-hole as the gases begin actively to work, and then the blast is reduced while the damper is somewhat lowered. Under this treatment, the ore mixture continues to do its work until the whole is reduced to metal. It asks for no puddling, no exhausting physical labor, but simply to be let alone to do its own work in its own way.

I may have been tedious in detailing the value I have discovered there is in a non-flowing slag, and also in so minutely describing the working of the process, but I have purposely elaborated this part of the subject, at the risk of wearying you, because I believe *the true secret* of successful commercial economy in working the direct method lies in this direction.

There is nothing new in fluxes, nor in moulding the mixture into shapes for reduction. The few alkali or acid materials cheap enough to be used for fluxes have all long ago been known, and one inventor after another has used them in varied proportions in moulded shapes, and still failed to demonstrate commercial economy in competition with other methods of working. Like my own earlier experiences, the successive steps to bring ore to metal direct have not been well understood. A non-flowing slag to protect from re-oxidation, and a quick, high heat, seems to be the pivot upon which commercial economy turns. By coupling this with that other quite as important discovery —namely, placing the several masses of metal mixture sufficiently apart so that the heat generated from one may impinge upon another— the general outline for practical working seems to be complete. A wide range of experimental tests with ore mixtures, in cases and without them, and in various furnaces, have gradually pointed out the successive steps by which to make the method simple and easy. Now heat after heat may be withdrawn from the furnace in less than two hours with the regularity of puddling, but with very much lessened cost.

Having succeeded in satisfactorily working ore-mixtures, my attention has latterly been directed to the utilization of puddle, forge and heating furnace cinder as well as roll scale and hammer slag.

Puddle cinder is a rich silicated protoxide of iron, containing from 50 to 58 per cent. metallic iron, 16 to 18 per cent. of silica, and from

1 to 2 or 3 per cent. of phosphorus, beside some other impurities. An *average* of this cinder, both here and in Europe, would probably prove it to contain not less than 52 per cent. metallic iron. This class of cinder, in this country, is quite high in phosphorus, because the pig metal is boiled purposely very hot in order to throw the phosphorus as much as possible into the cinder for the better purification of the iron.

Heating furnace cinder is not generally as rich as puddle cinder, still it averages a large percentage of iron, probably not less than 45 per cent., while some of it, taken from furnace bottoms, is even richer than puddle cinder.

Forge cinder, the refuse of charcoal scrap sinking fires, and from Catalan forges, is a richer iron oxide than puddle cinder, and besides contains a very considerable quantity of reduced iron blended with it, which, by the usual method of working charcoal forges, it is impossible to separate.

Roll scale is a pure oxide of iron, containing, as gathered up in the mills, about 70 per cent. of metallic iron. Although apparently little scale is made from iron in hammering and rolling it, yet in large mills the annual quantity, when summed up, would be quite surprising. Where puddling is carried on, scale is often used by the puddlers at certain stages of the boil, but even in many of these mills the scale is swept up and thrown with the cinder.

Hammer and roll slag are both rich in iron, averaging perhaps nearly as much metallic iron as heating furnace cinder, say 40 to 45 per cent.

It is quite worthy of note that all these refuse slags, so very rich in metallic iron, and produced in so very large quantities, are often wholly wasted, or, if utilized, only command a fraction of the market value which the same number of units of iron would realize if contained in ore. Most blast furnace owners would rather pay several dollars per ton for ores containing as low as 30 per cent. of metallic iron than to charge their furnaces with cinder, to be had for the hauling, that analyzed 54 per cent. of metallic iron. Owing to the alloy with phosphorus, and the very refractory nature of these cinders, furnace managers will at best only use them sparingly. Many who are proprietors of both rolling mills and furnaces prefer to waste their rolling mill cinder on the dump rather than use it in their blast furnaces, for fear of contaminating a superior grade of pig iron they aim to produce. Even in furnaces not very particular as to quality of

iron, the use of cinder is usually limited to from 10 to 20 per cent. of the charge, the latter being rather an unusual quantity for ordinary brands of pig.

Not less than 40 to 50 tons of puddle cinder daily made by the Philadelphia rolling mills is given away for the hauling and barely pays for the labor of transportation to cars a short distance from the mills, to be delivered to neighboring blast furnaces. By this process this wasted cinder could be daily converted into 12 to 15 tons of good wrought iron at low cost.

In former years cinder pig iron was a marketable commodity, very much in demand for working into cheap rails, etc., but now ingot iron and low steel is so rapidly pushing it away, and displacing puddled iron in rolling mills, substituting superior for inferior stock, that cinder pig iron is no longer in request. With an increasing demand for better qualities, the less cinder can be used in blast furnaces, and the more it will accumulate. In Staffordshire and many other parts of England, at the present time, cinder has fallen so much in demand for furnaces that many rolling mills are seriously inconvenienced by the large accumulation of cinder around them. Indeed in some parts of Europe, and even in this country, cinder has been used to fill up vacant lots and low places, and sometimes even forms the embankment of railroads. In Sweden rich forge cinders, unused, have been accumulating in enormous piles for 50 years.

I have now succeeded in working these cinders, so rich in iron, and rolling them to bars at one heat. I prepare this material with carbon and substantially the same cheap fluxes as used for ores. It is safe to estimate, under all contingencies, that one ton of muck bar can be made on a regular working scale from three tons of puddle cinder, deoxidized with bituminous or anthracite carbon in heats of from 500 to 1000 lbs., according to the size of the furnace.

During a series of experiments at Round Oaks, Staffordshire, England, in the past winter, I produced as high as 43 per cent. by weight of muck bars in 2¼ hours from ¾ cinder and ¼ scale, deoxidized with bituminous carbon. In this instance the muck bar, with one-half scrap, has lately been cut up, sunk in charcoal, forged, reheated, and rolled to 1⅛ inch wire rods, showing no greater waste than usually attends that mode of working iron. These wire rods were then reheated and rolled down to No. 6 wire. The wire was salt coated and in four passes was drawn to No. 12½, after which it was annealed

and meal coated, and in two more passes it was drawn to No. 16 wire. No better illustration than this can be presented to show the character of iron made from cinder by this method. The cinder from which this iron was made contained more than 2 per cent. of phosphorus, whereas an analysis of the iron showed it had been reduced to $\frac{38}{100}$ per cent.

Within three months, at the Phœnix Iron Company's works, 27,426 lbs. of puddle cinder, 7350 lbs. of old bed Champlain Ore, and 2400 lbs. of iron scale was mingled with bituminous coal and slagging material. The combinations were varied, and it was then moulded into shapes for testing the system. The charges were placed in an ordinary double puddling furnace. Some of them were of cinder alone, some cinder and ore, and others cinder and scale. In this way a variety of tests were made, each with a sufficient quantity to run the furnace several heats upon one mixture. Part of the time the furnace was run day and night continuously, making then 6 heats in 24 hours, averaging 507 lbs. of muck bar at a heat. Since then the heats have been much shortened.

The mixtures were all moulded 15 inches high into the annular cylindrical shape, being $8\frac{1}{2}$ inches outside diameter, and cored from top to bottom 3 inches diameter, and for more uniform heat circulation they were cross cored through and through at the base. With cinder mixture they contained each 48 lbs. of cinder, and of ore alone 52 lbs., while the weight varied between these points in proportion to the variation of the ore and cinder mixed together.

In every one of these tests the iron oxide was reduced to metal, balled, squeezed in a rotary squeezer and rolled to muck bar at the same heat, presenting in lengths and appearance the ordinary muck bar from pig iron. The average period of the heats was 3 hours, while the yield of the whole in weighed muck bar was about 32 per cent. of the weight of the oxide.

At the previous test, before referred to, the heat was $2\frac{1}{4}$ hours and the yield larger. This is accounted for mainly by the lesser thickness of the moulded shapes, which admitted more rapid heat penetration. The cost of moulding pipes of the less thickness by machinery is comparatively trifling in comparison with the quickened production and reduced cost of the metal.

A greater advantage will be gained in doubling the hearth capacity to twice that of the ordinary double puddling furnace. Then 1000

lbs. of iron, with no more labor, may be as easily produced at a heat as 500 lbs. has already been done with the present size.

Such increased furnace proportions would not be as economical for puddling pig iron, but as by this system the carbonic oxide generated by the metal mixture itself is largely depended upon as a heating power, and it is distributed over the entire hearth surface, the conditions are quite different from puddling pig iron. In this case the pure oxygen of the metal mixture from all parts of the hearth readily finds for itself (without furnace labor upon it) atoms of carbon in close contact with which to assimilate and generate heat.

In puddling pig iron the 2 or 3 per cent. of carbon it contains is prevented from escaping by the melted metal. Its exit is sealed, or at least very much retarded, by the *weight of the melted metal*, and it is only after excessively laborious "rabbling" or stirring that the way is opened for its escape. Thus allowed to volatilize, it combines with oxygen into carbonic oxide, but as three-fourths of the furnace gas is nitrogen, for which carbon has no affinity, it becomes necessary to pass large volumes of air very rapidly through the furnace, in order to have sufficient oxygen present with which the carbon may combine. With the swift movement of these air currents, sweeping like a whirlwind through the furnace, much of the carbon from the pig is passed off undecomposed, and along with it vast volumes from the fuel, rendering the top of the stack a constant monitor of wasteful combustion.

The following analyses have been made by the direction of David Reeves, Esq., President of the Phœnix Iron Company, by whose kind permission I use them.

An analysis of the 27,425 lbs. of puddle cinder referred to as recently worked proved its composition to be

Silica,	17·710
Iron,	54·290
Phosphorus,	1·960
Sulphur,	·280

The ore used was magnetic known as "old bed Lake Champlain," such as is usually used for fix, and highly charged with phosphorus.

Fluxes and bituminous coal were mingled in different proportions with a varied combination of cinder, scale and ore, and consecutively numbered.

No. 1 was composed of 6000 lbs. of cinder alone. An analysis of the 1690 lbs. of muck bar produced from it showed it to contain

Silica, ... ·48
Phosphorus, ... ·40

No. 2 was a mixture of two-thirds cinder and one-third "old bed Champlain" ore, making 6000 lbs. in all. The 1644 lbs. of muck bar it produced analyzed

Silica, ... ·45
Phosphorus, ... ·37

No. 3 was of 6000 lbs. of cinder alone, but fluxed differently from No. 1, and the analysis of its 1853 lbs. of muck bar was

Silica, ... ·35
Phosphorus, ... ·38

No. 4 was composed of 1200 lbs. "old bed Champlain" ore and 4800 lbs. of cinder. The 1842 lbs. of muck bar from it analyzed

Silica, ... ·55
Phosphorus, ... ·55

No. 5 was a mixture of 2400 lbs. iron scale and 4800 lbs. cinder. The 2183 lbs. of puddle bar it produced analyzed

Silica, ... ·29
Phosphorus, ... ·36

No. 6 was exclusively of 4673 lbs. of "old bed" ore, whereas the analysis of 1437 lbs. of puddle bar from it showed

Silica, ... ·62
Phosphorus, ... ·16

No. 7, with a different mixture of fluxes from any of the others, was composed entirely of 1887 lbs. of cinder. The 537 lbs. of muck bar analyzed

Silica, ... ·26
Phosphorus, ... ·38

Some important conclusions may be deduced from these experiments. For instance, in the mixture No. 3, the 6000 lbs. of puddle cinder contained, by analysis, 17·7 of silica, or 1062 lbs. of the entire weight, whereas the analysis of its muck bar showed it contained only $\cdot\frac{35}{100}$ of one per cent., or $6\frac{1}{2}$ lbs. This is an elimination of 1056 lbs. of silica from the 1853 lbs. of muck bar produced from this mixture.

Again, the analysis of this No. 3 puddle cinder showed it contained 1·96 per cent. or 117 lbs. of phosphorus, while the analysis of the muck bar contained only $\cdot\frac{38}{100}$ per cent. or 7 lbs. Here is an elimination of $110\frac{6}{10}$ lbs. of phosphorus out of $117\cdot\frac{6}{10}$ lbs. which the 1853

lbs. of muck bar, made from the 6000 lbs. of cinder, originally contained.

By referring to my first paper, it will be observed that an analysis of a *finished* bar, made by this process from Republic ore, showed an elimination of $\frac{7}{8}$ of the phosphorus, but the foregoing shows even a better result from the muck bar from *puddle cinder* carrying $1{\cdot}\frac{96}{100}$ per cent. of phosphorus, while *Republic ore* only contains $\cdot\frac{053}{1000}$ per cent. I deduce from this that the proportion of phosphorus eliminated, whether the quantity be large or small in the ore or cinder to be worked, will always be such *as to give value to a highly phosphoritic metal mixture reduced to iron by this process.*

This system is also applicable, not only for iron oxides, but for all other metallic oxides requiring reduction.

An examination of the entire seven analyses shows results substantially alike, and proves how thoroughly impurities are separated in a few hours by this method.

Here is another important deduction, proving that even low-grade ores may be profitably worked upon this system. The mixture No. 4 was composed of 4800 lbs. of puddle cinder and 1200 lbs. of "old bed" ore. To this 6000 lbs. of metal mixture was added $12\frac{1}{2}$ per cent. of earthy matter, so that the entire quantity of earthy matter contained amounted to 1617 lbs., while the 1842 lbs. of muck bar produced from it contained only ·55 per cent. or $10\frac{13}{100}$ lbs. of earthy residuum. An elimination of 1607 lbs. out of 1617 lbs. originally contained in the metal mixture.

This would show that ore or puddle cinder, containing 30 per cent. of earthy matter in all, can still have it eliminated down to $\frac{55}{100}$ of one per cent., and I believe by a slight increase of lime it may be *entirely separated.*

This fact is worthy of consideration in connection with the low-grade ores which so generally abound. Throughout the coal measures this class of ores are found, closely interstratified with the fuel, and by reason of this proximity produce iron at low cost. Yet because of the preponderance of sulphur and phosphorus, they are not satisfactorily worked alone in the blast furnace.

Turning to the recent Geological Survey of Pennsylvania, vol. "MM," page 179, I find an analysis of Westmoreland county ore containing 41 per cent. metallic iron and 50 per cent. volatile impurities and less than 10 per cent. of insoluble residue. This is a type of a large

variety of ores in Cambria, Huntingdon and, in fact, throughout the entire bituminous coal fields. Many of these ores contain more than ten per cent. of earthy matter, but rarely exceed 30 per cent., which has been referred to as so satisfactorily worked in the mixture designated as No. 4, where the earthy residuum was reduced to $\cdot\frac{55}{100}$ of one per cent.

The facility of eliminating phosphorus so largely from puddle cinder containing nearly 2 per cent., as has been proved by the foregoing analyses, should be quite conclusive that the phosphorus can be readily removed from these ores of the coal formation when muck bar would then be produced from them at one heat not only at the lowest possible cost but of a very superior quality.

I have dwelt upon this branch of the subject because of its economic importance. If the gaseous and earthy impurities can be so easily removed from low-grade ores, as the result of the analysis of the No. 4 mixture would seem to prove, then the field of usefulness of this system will be much enlarged.

Within the last two weeks, still more satisfactory results have been obtained from working the charcoal-forge cinders at the forge of the Washburn & Moen Manufacturing Company, at Quinsigamond, Massachusetts. From these cinders, 1794 lbs. of blooms were produced from a small sand bottom reverberatory furnace, there being none there with cinder bottom.

The average yield, from the entire quantity of cinder used, was 38 per cent. of its weight in forged blooms. *It was not unusual to shingle blooms of 150 to 180 lbs. in one hour from the time the mixture was charged in the furnace, while the average length of the heats did not exceed 1½ hours.* Four heats were made between ten o'clock and five, being an average of 1¾ hours, including the usual delays in repairing a sand bottom. Deoxidation was accomplished by using the fine breeze or dust charcoal, which is now wasted at charcoal forges. In this instance the tubes were 15 in. high, 8 in. diameter and 2¾ in. thickness.

The blooms thus produced, while still hot, were for a few minutes reheated and then rolled to muck bars. These bars were cut up and, with an equal quantity of scrap, sunk in the charcoal-forge fire, then forged, reheated and rolled down to 1⅛ inch wire rods, with no more than the usual waste in sinking iron in charcoal fires.

The rods so produced have all been rolled down to No. 6 wire, and part of it has been drawn to No. 12 size, with the intention to draw it

still finer if the stock will permit. Both the analyses, tensile strength and various further tests of this forge cinder iron is expected to be fully obtained within the next few weeks by this company.

The few experiments herein described have been the first attempt to make wire from cinders to my knowledge, and it is possible that the practical working of it may prove that the metal mixture was not sufficiently *basic* to eliminate enough silica and phosphorus to produce a very fine wire. Should such be the case, the difficulty can easily be removed in the future by slightly increasing the proportion of lime.

There are several elements of economy by this method that cannot be attained by producing pig iron from ore and then puddling it. To produce pig, it is essential to roast refractory and very impure ores and cinders before charging them in the blast furnace. This is required in order to expel volatile impurities as far as possible, as well as to render them more porous for the mingling of the gases.

Roasting is unnecessary for this direct method. By grinding the mixture together, properly proportioned, there is such close *atomic contact* created between the particles that reduction takes place rapidly without roasting. This is one element of economy.

Usually the mixture has been ground as fine as coarse Indian meal, but in the last experiment with forge cinder at Quinsigamond, just referred to, one of the mixtures was composed of one-half the cinder crushed to the size of small beans and the other half as fine as meal. The yield in metal and length of heat did not vary from that of the other forge cinder mixtures there, which were all ground fine, proving that very fine pulverization is unnecessary for forge cinders.

Another element of economy is in the item of fuel. Blast furnaces using bituminous coal require it to be coked, the better to carry the burden and secure greater purity of the gases from it. This operation of coking wastes a large portion of the combustible gases which should be used in reduction. Coking is unnecessary by this new system. Its cost and waste is saved. Bituminous coal, both for deoxidizing and fuel, is used without coking, and repeated analyses prove the iron is freed not only from its own impurities, but that it is uncontaminated by the impurities of the coal.

Still another economy may be counted upon. It is necessary to run the liquid pig iron from the blast furnace into moulds, which are freshly prepared, at considerable expense, for every cast. By this direct system, the metal mixture, *automatically ground, mixed and*

moulded, is charged at once into the reverberatory furnace as has been explained. The entire cost for labor up to this point is less than blast furnace labor.

Pig metal must be broken before charging. It comes to the puddling furnace cold, must be reheated and rabbled with exhausting physical labor, and at an intense heat, in order to separate the intimately combined carbon and silica, before it is changed to wrought iron ready for the squeezer and rolls.

By this system the metal mixture is reduced to a wrought or malleable condition at one operation from the ore. In 1½ to 2½ hours, according to the thickness of the moulded shapes, it is ready for the squeezer and rolls, and is brought to muck bar. It is then not only of superior quality to puddled iron, but is one step in advance of it in the process of manufacture; a step which requires from $12 to $15 per ton outlay on the pig iron to bring it to a malleable condition.

If the balled iron has been made from material nearly free from phosphorus, without squeezing, in its heated state, it may be transferred to and melted in the open hearth bath for steel, when it will separate from its earthy impurities by precipitation. If, on the contrary, the stock originally contained an objectionable percentage of phosphorus, the slag into which the phosphorus has been incorporated must first be squeezed out to guard against a second combination with the metal. Then the bloom, in its then highly heated state, may be at once melted in the bath, with the certainty of being freed from phosphorus. In either case a *superior, cheap and uniform quality* of metal is assured, adaptable, at pleasure, to any purpose required, from the best ingot iron, low in carbon, to the finest grades of high steel.

Practical tests, on a regular working scale, have thus far proved that the metal by this system will produce the finest grades of crucible, or open-hearth steel. Sunk in charcoal it makes fine grades of tough sheet iron, both of black and planished iron. Tests, not yet fully completed, are sufficiently advanced to prove that it will make a good quality of wire. In addition to this, as it can be proved that its cost is much below the cost of ordinary scrap or puddled iron—that its mode of working is simpler—that the cost for plant for a given quantity of iron or steel is very low—and that impure ores or cinders may be worked with safety, the process may reasonably commend itself to the close investigation of manufacturers.

DISCUSSION

*Of the Papers of C. P. Sandberg on "Rail Specifications and Rail Inspection in Europe," of C. B. Dudley on the "Wearing Capacity of Steel Rails in Relation to their Chemical Composition and Physical Properties," and of A. L. Holley on "Rail Patterns," at the Philadelphia Meeting of the American Institute of Mining Engineers, held at the Franklin Institute, February 17th, 1881.**

ASHBEL WELCH, Lambertville, N. J.—Dr. Dudley has given the wear of steel rails under four different conditions. He arrives at the conclusion that the softer rails, or those that from their composition ought to be softer, wear better than the harder. But there is another condition which has an important bearing on the subject, and should not be overlooked—the weight on a wheel. With the lighter weights of the past, the softer rails may have worn best; with the heavier weights of the future the harder may wear best. Weights will probably be increased up to the capacity of steel to bear; then, doubtless, the harder steel will wear best.

A leaden rail, with 10 pounds on a wheel, might carry millions of tons, but with 100 pounds on a wheel it would be destroyed by a few thousand tons. So in the days of iron rails, my experience was that the softer rails under light machinery stood better than some of the harder; but under heavy machinery the softer were much the most rapidly destroyed. It is doubtless the same with steel.

The pounding motion of the wheels loosens or spreads the particles of a thin film of steel; the pull lengthwise on the rail detaches or scrapes them off. The softer the metal, the more liable the particles to spread or flow sideways; the more brittle, the more liable the particles to break loose. With light machinery, flowing may be practically nothing, with heavy machinery it may be enough to wear the rail out

*The remarks as here given, as in the previous discussion of Dr. Dudley's papers, have all been written out or revised by the participants in the discussion, and represent, therefore, their mature views. It has been thought that this plan, when it can be carried out without doing any of the speakers injustice in debate, is much to be preferred to a strictly verbatim report. The remarks of Mr. Chanute were sent to the Secretary after the meeting, and although they did not form a part of the actual discussion, there can be no doubt of the desirability of including them in this report.

For Dr. Dudley's paper see JOURNAL for March and April, 1881.

very rapidly. At a certain point doubling the weight might increase the flow tenfold. The harder the metal, without decrease of tenacity, or increase of brittleness, the better we should expect it to wear. All may depend on what is left in, or used to make it hard.

Dr. Dudley's observations give us incidentally the difference in wear per million of tons carried, caused by difference of weight on a wheel. On the south or loaded track the gross tonnage was 8,000,000 per annum, on the north or light track 5,000,000. As about the same number of wheels must have gone one way as the other, the average weight on a wheel must have been 60 per cent. more on the loaded track than on the light. The wear per million of tons gross load, as found by summing up the wear on each rail, averages 31 per cent. more on the loaded than on the light track. So, in this case, the wear per ton of gross load increased as the 0·6 power of the weight on a wheel. With iron rails in former years it increased much faster than this rate. As machinery becomes heavier it will doubtless increase faster with steel.

As the same engines and tenders with the same weights on a wheel passed over each track, and as the speeds were probably greatest on the light track, the difference in wear due to difference in weight on the freight-car wheels was probably greater than above estimated. The former weights on a freight-car wheel was about $\left(\frac{20000+20000}{8}\right)$ =5000 pounds. The weights now coming into use are about $\left(\frac{24000+40000}{8}\right)$ =8000 pounds, an increase of 60 per cent. over the weights when the wear reported took place. This may entirely change the relative rates of wear of hard and soft steel.

Several interesting inferences seem to be deducible from Dr. Dudley's observations, but I confine myself to the single point I have made. Ten years ago I found the wear of steel on the roads between Philadelphia and New York about 50 per cent. more than Dr. Dudley found it on his road. This is accounted for by the better road-bed and smaller proportion of passenger trains at high speed on the Pennsylvania Railroad, by the narrow-gauge cars on wider gauge roads as it then was, and especially by the steel rails between here and New York being then only in the worst places. The softer Sheffield rails, made from Swedish pig, wore better than either of the two harder kinds I got from France.

Dr. Dudley's conclusion seems to be that rails should approach the condition of Bessemer iron. I found that the wear of iron rails from Bethlehem was about 25 per cent. more than that of steel from Sheffield, laid in the same track and under the same circumstances. This, however, does not show the relative durations of the rails, for steel, owing to its elasticity, is only injured on the surface, and will wear till it is reduced to a skeleton; while iron is affected all through by each blow, and will finally go to pieces before it is worn down. The point I make is, that though the softer steel may have worn best under the lighter machinery of the past, it does not follow that it will wear best under the heavier machinery of the future.

The patterns of steel rails now displayed, and the papers of Mr. Sandberg and of Mr. Holley, with the allusion that has been made to the history of the now accepted forms, make it proper to give a short historical notice of the first rails which had certain characteristics common to all these patterns, and of the principles on which they were made.

The early steel rails, copied after the iron, had very heavy bases and stems, and no flat surfaces for fishing. One-fifth to one-eighth of the metal put into them did little or no good, and the fish splice, then coming to be recognized as the best, could not be used to advantage. The useless weight made steel rails so expensive that they came into use very slowly. In 1865 I made a pattern to avoid these faults, guided by the following considerations: The theory then was, and I suppose is yet, that a blow on iron such as that given by a locomotive wheel, is felt all through the metal, and produces a permanent though minute disintegration or change of form, the accumulations of which must in time weaken and *unweld* the base and stem as well as the head of the rail, and so extra metal must be put into the lower part of the rail to compensate for this gradual weakening; but that in steel, owing to its elasticity, such a blow produces no permanent effect on the metal, except at the surface. Therefore, the stem and base of steel may be very much lighter than of iron, not only on account of its greater strength, and freedom from welds, but also from its immunity from deterioration by use. Hence, all the metal possible should be in the head where the wear is; and as little as consistent with safety in the stem and base where there is no wear, and with steel no deterioration. In England, where the supports are far apart, strength and stiffness are primary considerations; in this country, where the supports are

close together, other considerations engross attention. The width of the base should be determined by the endurance of the wood it is to set on, and for this purpose must be much greater than is necessary for strength or to prevent upsetting. Partly from calculation and partly from observing the behavior of iron and steel in circumstances somewhat similar, I made up my mind that three-eights of an inch thickness of stem was ample to bear the weights and the shocks, vertical and lateral, of the machinery then in use, and that an eighth thickness at the edge of the base was sufficient to transmit to the wood all the pressure its fibres would bear.

In accordance with these views, but conceding something for the sake of abundant safety and the facility of manufacture, I made a pattern with 4 inches height, 4 inches base, head $2\frac{3}{8}$ wide$\times 1\frac{1}{4}$ deep, stem 7-16 thick, and edge of base 3-16 thick, weighing 53 pounds to the yard. Assuming that fishing made the best joint, I made (as I had previously done in very slender iron rails) the under side of the head and top of the base plane surfaces, as broad as possible, so as to give a perfect and broad bearing to the edges of the fish-plates, and as near horizontal as possible, so as to lessen the tendency of the fish-plate to work out by the jar.

This pattern was condemned by every engineer to whom it was shown, on the ground that it was too weak, and could not be rolled. I was fully aware of the difficulties of manufacture, but was confident they could be overcome. In one point I yielded too much to the manufacturers—rounding off the corners on the under side of the head.

After long negotiations, an order for 200 tons for trial was accepted by Naylor & Co., in August, 1866, and sent to England to be executed. There everybody that saw it condemned it, and for several months John Brown & Co. refused to roll a rail of such a preposterous shape. As I wished to have an extreme test of the correctness of my ideas, I insisted on the performance of the contract. At last the rails were rolled, tested by Kirkaldy in London, and in the spring of 1867 laid down in some of the hardest places between this city and New York. Several hundred tons of the same pattern were laid the next season. I continued to watch and test them carefully for eight years, and so far as I could find not one ever broke, bent or compressed in the stem, or gave out in any other way.

Thus the principles on which this pattern was made, and its proportions with the machinery then in use, were shown to be correct as

seen from the consumers' point of view. It had as big a head and wore as long, and was as free from accident as previous steel rails 20 per cent. heavier. Some manufacturers in this country condemn the pattern because, without trial, they believed it could not be safely rolled. But John Brown & Co., after trial, believed it could be, and showed their confidence by soliciting an order for 10,000 tons more of the same pattern.

The principles of this pattern, with the proportions slightly modified, were gradually, and are now generally, adopted. Mr. Hinckley, after a year's observation, adopted the pattern with a very slight addition to the metal below the head, and in 1868 relaid one track with it from this city to Baltimore, where the rails may still be seen.

In 1874, Mr. Chanute on the Erie, and Mr. Sayer on the Lehigh Valley, simultaneously, and without concert, adopted the sloping sides of the head, which important feature is now in general use.

In 1870, Sandberg's patterns were published, embodying the same principles with the angles and proportions slightly different. Whether he knew what had been done before I do not know. Doubtless the same considerations on which I acted occurred to many others.

As the machinery is now much heavier than in 1866, and steel not now so good as that made by John Brown & Co. from Swedish pig, and as the price of steel is very much lower, a small saving in weight is of less importance. I have nothing to say against the somewhat heavier proportions now generally used; for example, $\frac{1}{2}$-inch stem instead of $\frac{7}{16}$ and $\frac{1}{4}$-inch thickness at edge of base instead of $\frac{3}{16}$.

In Sandberg's new patterns of 1878 he has, however, adopted almost the identical proportions I used in 1866. His thickness of stem is exactly, and thickness of the edge of the base is very nearly the same. His fishing angle which, in 1870, was 22°, is now 30°; mine was always 28°. It is gratifying to find that so able an engineer, after so wide and so long experience, has within the last three years settled down upon the proportions I adopted fifteen years ago, and which, before they were tested, were so universally condemned, especially in the very dimensions now adopted by him.

I think Sandberg's heads are too convex, and his bases rather narrow. In America bases are always far wider than stiffness and stability required, the practical question being, What width of bearing does the timber require? Where chestnut ties are used the base should be

not less than 4½ inches. There is, therefore, little practical relation between the height and base.

The old plan was to increase every part of a rail much in the same proportion; but each part should be in proportion to what it has to do. The head should be deep in proportion to the amount of traffic and the lowness of the rate of interest on its cost. The body need only be strong enough to carry the head after it is well worn down, and that depends on the weight of the machinery, and in the case of steel has little to do with the volume of traffic, except so far as that affects weight of machinery. As on most railroad systems, the same machinery is used on main lines with heavy traffic, and branches with light, I suggested, in 1874, that each system adopt the same body of rail for both, and make the head deeper on the main lines, shallower on the branches. This plan was adopted on the Pennsylvania Railroad. Sandberg seems to recognize this in his patterns of 1878.

R. W. Hunt, Troy, N. Y.—Again Dr. Dudley presents to our consideration a series of most carefully-conducted experiments, and while I fully appreciate the labor and thought this work has cost him, I must still hesitate to accept his deductions.

This paper differs from the previous one in that it deals exclusively with the wearing qualities of steel rails. Dr. Dudley gives his reason for this change in the following statement: "With the improvement in maintenance of way which has characterized the Pennsylvania Railroad during the last five or six years, the removal of rails from the track from the first two of these causes (*i. e.*, broken and crushed) has, if I am right, quite notably diminished. This certainly is true with regard to broken rails. And if, as time advances, the number of crushed rails shall diminish, both because of the continued improvement in maintenance of way before referred to, and because, owing to improved and better methods at the steel-works, there are fewer crushed rails caused by physical defects in the steel, the question of the wearing capacity of steel rails obviously becomes the all-important one."

Certainly, the condition of the road-bed has much to do with rails breaking, crushing and wearing. If the Pennsylvania and other railroads have done and are doing their part, Dr. Dudley courteously admits that the steel-works have performed part of theirs. But, as far as I am informed, the formulas which they have used have been, and are still, quite wide of the one he continues to recommend. The

rails which are not breaking or crushing to so great an extent as formerly contain higher percentages of both carbon and manganese. I will venture the assertion that Dr. Dudley's road has put in but few rails during the last eighteen months that have not contained fully 0 35 per cent. of carbon and 1 per cent. of manganese. And probably the use of this formula will considerably ante-date the time mentioned. These rails do not break or crush, because they were laid upon a better road-bed, were rolled from sounder ingots, were carefully hot-straightened, and, if I may be permitted a Hibernianism, were cold-straightened while still hot. But how will they wear? For an answer to that we must wait.

I have endeavored to study carefully Dr. Dudley's paper, but have failed to be convinced of the correctness of the conclusions which he draws from the chemical analyses and physical tests. I think I have no prejudice in this matter. The best formula for steel for rails is as earnestly desired by me as by any consumer of such steel. In my judgment, averages in such investigations are exceedingly dangerous, unless made from an immense number of samples taken from metal that has had exactly the same history. By this I mean the different samples ought to have been blown at about the same temperature, cast under the same conditions, heated alike, rolled at the same heat and under the same reductions, hot and cold finished alike, placed upon the same road-bed, and given the same amount and kind of wear. This is almost an impracticable proposition, but the failure to fulfill it can only be compensated by an immense number of other samples.

Some of us told Dr. Dudley before that his twenty-five samples were too few upon which to build up a theory, and it seems a little ungenerous to make the same charge against his present sixty-four; but I must do it. These sixty-four tests are not taken from rails which have been subjected to the same conditions; on the contrary, "sixteen of these rails were taken from level tangents and sixteen from level curves, eight from the high side and eight from the low side of the curves. Again, sixteen rails were taken from grade tangents and sixteen from grade curves, eight from the high side and eight from the low side of these curves." Asking for so great a number of tests means a tremendous amount of work and patience, but Dr. Dudley has taken up one of the most difficult problems, and must not be contented with a partial investigation. It took some of us a long time to learn how

to make Bessemer steel at all; he must not expect to be able to so easily teach us how to make the best.

To illustrate why I object to his averages, I find among the rails taken from a level tangent that the one which shows the least loss in section and the least wear per million tons of traffic had carbon, 0·423; phosphorus, 0·127; silicon, 0·083; manganese, 0·708; while another rail presenting within three of the worst results, had carbon, 0·428; phosphorus, 0·109; silicon, 0·038; manganese, 0·870. The next poorest had carbon, 0·452; phosphorus, 0·144; silicon, 0·037, and manganese, 0·708, the manganese being exactly like the best. The poor steel had also very slightly the greatest density. The difference between 0·452 of carbon and 0·423 and 0·428 could easily be caused by rolling one steel at a higher heat than the other. Again, on the low side of level curves I find the second best rail had carbon, 0·454; phosphorus, 0·145; silicon, 0·015; manganese, 0·726, while the poorest rail had carbon, 0·497; phosphorus, 0·136; silicon, 0·062; manganese, 0·724. I cannot now believe that the slight difference in the chemical constituents of these rails caused the great difference in their wear. If such is the case, then I for one stand appalled at the difficulties which surround the making of a perfect rail.

As before stated, averages are dangerous. Dr. Dudley makes up a formula from the averages of his investigations, but admits that the silicon percentage is disturbed by an abnormal piece of steel, No. 881. This had carbon, 0·483; phosphorus, 0·035; silicon, 0·480; manganese, 0·782, and stands eighth in sixteen tests. It may be remembered that in the discussion at the Baltimore meeting in February, 1879, I mentioned a rail then in the track of the Boston and Albany Railroad that contained carbon, 0·360; phosphorus, 0·124; silicon, 0·469; manganese, 0·571, and which had then been in the track five years. That rail is still in service and in good condition. Here we have two rails made by widely-separated works, laid in the tracks of railroads hundreds of miles apart—one giving eleven years and one month of service, and then not worn out, but, on the contrary, selected as a "slow-wearing rail"; the other one good after over seven years of wear. Will we take the average composition of these rails which did not break, nor crush, nor rapidly wear out, and assume that carbon, 0·420; phosphorus, 0·079; silicon, 0·474, and manganese, 0·676, is the proper formula for rail steel? If not, why not?

Dr. Dudley's averages of his 32 samples of good wearing rails gave

him carbon, 0·334; phosphorus, 0·077; silicon, 0·060; manganese, 0·491. If that formula is right, stick to it. In fact, such a one will be better for both the producer and consumer than the compromise which Dr. Dudley recommends. I think every steel maker will bear me out in saying that sound steel can be more easily made under its provisions than with carbon from 0·25 to 0·35; phosphorus, 0·10; silicon, 0·04; manganese ranging from 0·30 to 0·40, aiming at 0·35. Theory is very fascinating, but in practice stubborn facts present themselves, and with steel containing 0·10 phosphorus, and not more than 0·35 manganese, the resulting ingots would be very unsound, and the rail-mill would produce an indefinite number of imperfect rails, many of which would get into service in defiance of the most careful inspection, the result being crushed ends, flat places, and generally unsatisfactory rails. If low manganese is desired the phosphorus must also be low; 0·10 per cent. cannot be so considered. In the 64 analyses there are but 16 with the manganese as low as 0·40 and under, and only 4 of these have the phosphorus above 0·085, 11 being under 0·07 and 6 under 0·05. The rails having less than 0·30 carbon, with the exception of six, were made over 12 years ago, and of these six, one was made 8 years, one 10 years and four about 11 years ago.

At the time all of these rails, excepting one, were made, all steel was hammered, the blooming-mill not having been invented. Under the hammer it is possible to coax steel into fair-appearing blooms that would either go to pieces, or roll very badly in the blooming-mill. When the latter was introduced the steel-makers had only at their command recarburizers poor in manganese and high in phosphorus. Moreover, the American irons were then even much higher in phosphorus than our chemists told us; hence, a great deal of very poor steel was made—by poor, I mean unsound steel. As high as 20 per cent. second quality or defective rails was a common run of work, while to-day, with better irons, richer spiegels and better melting-furnaces, we rarely exceed 1 per cent., and run for days at less than one-half of that figure, and this on much more difficult sections than railroad engineers formerly required.

Just here let me ask Dr. Dudley whether he gave sufficient consideration to this very difference of section. Examples are presented by rails 902 and 903. The first stands at the head of rails from the high side of curve-grade, and gave, upon analysis, carbon, 0·322; phosphorus, 0·077; silicon, 0·026; manganese, 0·492. The second had

carbon, 0·355; phosphorus, 0·108; silicon, 0·029; manganese, 0·490, and is at the foot of the list with but 2 years 11 months service, while the first had 7 years 11 months, and was of the old rounded head section, while the poor one, with almost the same chemical analysis, was of the square head pattern.

If, as suggested by Dr. Dudley, even softer rails are desirable, there need not be any difficulty in filling such an order. I would be perfectly willing to contract to make rails containing not over carbon, 0·15; phosphorus, 0·08, and manganese, 0·50. These rails would be perfectly homogeneous and stiffer than an iron rail of the same section, and ought, therefore, to hold up the load. The rail-makers, both in this country and England, are now making steel containing much more manganese than formerly. These rails are going out of the mills apparently better and sounder than those formerly made. Time only will demonstrate whether or not they are actually better.

The present Troy practice is to use irons containing the least phosphorus, to put in enough carbon to make strong steel, and enough manganese to make the steel roll sound, both while in the ingot and the bloom, to carefully heat the ingots and resulting blooms, hot straighten the rails so as to leave the minimum of work for the cold press, which does its work while the steel is yet hot. And we have to be yet convinced by the wear of our rails that this practice is wrong. For our tests we cast a 4-inch ingot from each blow; this is hammered into a $\frac{3}{8}$ inch bar, which, when cold, is required to bend to at least a U by the blows of a sledge, this bending being a much severer test than when done in a press. The steel is also quenched in water and tested for temper. Drillings are taken from the ingot, and *accurate* carbon determinations made.

I prefer this plan to any test of the rail ends. To be perfectly conclusive, such tests would have to be made of both ends of the rail, and from every rail, for one end might be overheated and the other not. Some blooms might be all right, and the rest of them heat-spoilt. In a mill producing, say 9000 rails per week, 18,000 rail-ends tests would be no inconsiderable item, particularly if, as Dr. Dudley proposes, the test-piece, "12 inches long, $1\frac{1}{2}$ inch wide, $\frac{1}{2}$ inch thick," is to be slotted from the web of the rail. He would have to give a lease of the Altoona shops along with the rail contract.

Dr. Dudley's theory of infinitesimal teeth is interesting, and, if true,

WILLIAM SELLERS, Philadelphia.—The very interesting paper upon the wear of steel rails that has just been read presents the record of a series of investigations that are extremely valuable, and the deduction that has been drawn from the results noted seems to be unavoidable; the tests, however, to which it is proposed steel rails shall be subjected hereafter, with a view to determine their quality, should have our careful attention, and with reference to these I desire to make a few suggestions.

While the manufacture of soft steel was yet in its infancy it was believed that the presence of phosphorus in any notable quantity was very deleterious, in fact fatal, to the good quality of this product, and the greatest care was exercised to procure materials in which this element could not be more than traced. With the development of this art it has been found that larger and larger proportions of this hurtful ingredient may be used, providing always corresponding changes in the chemical composition shall be made to accord therewith.

It is, perhaps, besides the point to inquire whether the earlier belief was correct or not, the fact remains that steel is now produced which contains much larger proportions of phosphorus than would have been permitted a very few years ago, and that this steel is now considered to possess qualities which fit it admirably for use in rails; moreover, it is well known that the degree of heat, and the manipulation to which the ingot is subjected in transforming it into the finished product has an important influence in determining the characteristics that product will exhibit.

These facts have an important bearing upon the question, What shall be the tests which are to determine the quality we desire to attain? If the engineer is to specify the chemical composition, and the mode or process of manufacture by which the manufacturer must work, it would seem that improvement in the art must, to a certain extent, be limited, and a vicious system would be introduced, injurious alike to the engineer and the manufacturer. The chemical composition has no value to the engineer, for no matter what the chemical composition, it is upon the physical qualities at last that he must rely to determine whether or not it will answer his purpose. It should be his business, therefore, to devise such physical tests as will determine absolutely whether or not the quality that he desires has been produced, while the manufacturer should be left free to make such chemical combinations or adopt such processes of manufacture as will fulfill the requirements of

the engineer. It may, however, be questioned whether the physical data we now possess will enable us to agree upon the physical tests requisite to demonstrate the quality; but if this is admitted it only proves that further physical data are wanting, for the quality is finally determined by use, the result of which our physical data should enable us to predict. Of these data the one most abundant is the ultimate strength, and next to this ductility, bending, shearing, punching, torsion, impact and fatigue, in all of which, except the last, abundant facts are at hand. As most specifications prescribe a high and a low limit for ultimate strength, it would seem to be the prevalent opinion among engineers that high or low steel, as it is technically termed, is to be determined by its ultimate strength; and it becomes important at this stage to define what quality it is that most accurately defines this term, that is to say, does it consist in high or low ultimate strength, or in high or low ductility relatively to its ultimate strength? It is essential for all structural uses that the engineer should know upon what ultimate strength he can rely, for upon this all his calculations must be based; but it is upon ductility that he must depend for safety, the measure of which he must determine, after which the higher the ultimate strength he can obtain the better his material will be for any structural purpose; that is to say, with a given ductility the higher the ultimate strength of his material the safer it is, and conversely, with a given ultimate strength, the higher the ductility the safer it is. High ductility and high ultimate strength cannot be produced except with the most favorable conditions, both as to chemical composition and as to the mode of manufacture; the quality is, therefore, to be ascertained with most certainty by determining the relation of the one to the other, and for the same reason this relation would seem to be the factor which should most accurately define the term high or low, as applied to steel, and the requirements simply of an ultimate strength not less than ——— pounds per square inch, with a ductility not less than ——— per cent., would at once determine the character of steel required and the quality of it. It must not be understood, however that the determination of this relation is the sole requisite in determining quality for every purpose, for the capacity to bear fatigue and shock is scarcely less important, as, for example, the question now under consideration. And although it is probable that the material which exhibits high ultimate strength coupled with high ductility will prove to be capable of enduring the most fatigue and shock,

we cannot affirm that there is any definite relation between the two, in fact we have many data tending to show that such a relation does not exist.

An examination of the data which Dr. Dudley has tabulated indicates that to establish the relation between ultimate strength and ductility alone would be insufficient to determine the wearing quality of rails, so that these data must be supplemented by some other. This other, I suggest, should be that of fatigue from shock, not that of simply bending, which last Dr. Dudley "has found to bear a closer relation to the loss of metal per million tons than any of the other tests." I take exception to the classification of this test as one of the four ways in which a bending test could be applied. A rail bent under the drop test, and one bent in the testing machine by pressure slowly applied, would not be subjected to the same character of strains. While a drop test is a bending test, it is also much more; the same number of degrees of deflection in the one case as in the other would, I think, represent very different powers of resistance in the material operated upon. With the same weight falling from the same height in properly constructed guides, the same *effect* must be produced with every blow. In fact, it is difficult to conceive how any other form of test can produce more uniform effects or which can be more accurately measured; the *results* may be more diverse with such a test than with others, because they are produced by pressure and shock, whereas nearly all other forms of testing produce their results by pressure alone. It is this difference, however, which commends the drop as the test above all others for rails as being more analogous to that by which the rail is tested in use, and we should be well satisfied that the objections urged against it are well founded before we abandon it for others which may appear to offer more uniform results. It may well be that uniform results obtained by a system of testing widely different from that we require our material to sustain in use may have small value for determining in advance the effect of that use. I am thus driven to the conclusion that to obtain the relation of ductility to ultimate strength, together with the capacity to sustain fatigue from shock, would be to attain to absolute certainty as to the quality in an engineering sense.

There are, however, considerations other than that of the tests which must have attention before adopting a system of testing for steel rails, and as to these I would now make a few suggestions. The tests

required to determine quality in the directions indicated are well understood, but, simple as they are, the time that must be consumed in making them would result in serious loss to the manufacturer if his mill is to be held for their determination, and if he proceeds with the execution of his order in advance he incurs a serious responsibility in assuming the risk of rejection for the large product that would be turned out before the requisite tests could be made. While the cost of a test for ultimate strength, ductility and for capacity to bear fatigue would be small, the large number of such tests that would be necessary to establish the quality of an ordinary order for steel rails would be a serious item, and as every item of cost must be eventually borne by the consumer, it is important for the railway companies to adopt a regular system for testing their rails that shall not only be the most expeditious, but the least expensive. For the purpose of inspection, therefore, it would seem to be sufficient to adopt a system that would be simply an indication as to the qualities desired, without subjecting the parties interested to the cost and delay which must result from exhaustive and thorough tests, and upon these indications the rails might be accepted. This would seem to accord with the best foreign practice, as illustrated by the very admirable paper upon "Rail Specifications and Rail Inspection in Europe," by C. P. Sandberg, C.E., read at the Lake Superior Meeting, August, 1880. There are two tests which would give these indications with great accuracy, both of which could be applied without the expense and delay incident to preparation of specimens, and both of which require comparatively inexpensive machinery. These are the registering punch and the drop test. The former is a special tool which could be applied upon the crop ends, and could be portable; the latter is too well known to require description, but its indications if carefully noted would, I believe, be the most valuable of the two, but in conjunction with the punch they should be conclusive. The punching test would have this advantage: that by its use an inexpensive indication could be had of the quality of every rail. The suggestion that this test should be applied upon the fish-plate holes has probably prevented its introduction heretofore; first, because such holes are not now punched, and second, because to make such registration every manufacturer would have to procure a registering punch, and this would be difficult to apply upon existing machines. If this tool should be recognized as a part of the inspector's outfit and

specially adapted to his needs, it might soon come into general use if care was used to maintain the punches and dies in good condition.

In conclusion, I suggest that if the physical tests are to be supplemented by chemical analysis the specification for this analysis should not be complete; that is to say, in place of giving the proportions of carbon, phosphorus, silicon and manganese, a maximum limit should be fixed respectively for phosphorus, silicon and manganese only, leaving the carbon to be varied by the manufacturer, so that he may properly be required to furnish material that will fulfill the physical conditions; for it is evident that if the engineer defines the chemical composition he cannot reasonably ask the manufacturer to gurantee that this composition shall give certain physical results.

W. R. Jones, Pittsburgh, Pa.—The question that naturally occurs to me is this: Has Dr. Dudley in his investigations been aiming to prove a theory, or has he been guided by an earnest desire to discover what are the proper elements in the composition of a good-wearing steel rail?

Unfortunately, for correct chemical information, he has omitted in his analyses two very important elements—sulphur and copper. Now, before we will even begin to admit the correctness of Dr. Dudley's conclusions and the formula he prescribes, we will at the start question the propriety of any chemist or scientist prescribing a formula for making steel when he has ignored such important elements as sulphur and copper. I, for one, will not accept any such formula.

Are we sure, or is Dr. Dudley sure, that the chemical analyses embodied in his paper are correct? This may seem a presumptuous question; yet, with my experience with chemists, I naturally doubt the correctness of the analyses, and, before I will accept them as correct, I will ask that comparative tests of phosphorus and manganese be made by the Pennsylvania Railroad chemists and the chemists of the leading steel-works in the country. Let us first verify the correctness of the analyses before we consider the conclusions. I can enumerate a great number of instances in which chemists have differed very widely in their determinations of phosphorus and manganese. A prominent iron firm made a contract with the Edgar Thomson Steel Company to deliver pig metal guaranteed to be between 0·07 and 0·08 phosphorus. An analysis by our chemist resulted in phosphorus 0·148 and 0·152—a rather startling difference! Again, a sample bar

of steel, in which our chemist reported phosphorus 0·11, was tested by a chemist of another Bessemer works, and his determinations were phosphorus between 0·07 and 0·08. A leading engineering establishment of Pittsburg bought iron claimed by a chemical analysis to contain 0·08 phosphorus; our Mr. Ford found phosphorus 0·145. A chemist connected with an open-hearth works reported manganese in a piece of steel, 1·14; in a second determination from the same piece of steel, by the same chemist, manganese was reported 0·43. The chemist was kept in ignorance of the fact that both samples were from the same piece of steel. Two determinations for manganese were made by the same chemist from a piece of steel; he reported manganese 0·61 and 0·58. Our chemist, in the same steel, reported manganese 0·324 and 0·303. I could give innumerable instances of the wide difference in chemists' determinations of phosphorus and manganese. I think I have cited sufficient cases to sustain the position I have assumed, viz., that before the determinations made by Dr. Dudley's assistants are accepted as being correct they should be verified by chemists of greater experience.

I find on a close examination of the doctor's paper that he has taken no notice whatever of the increased weight of cars, increased weight of locomotives, with increased speed, in his tonnage calculations. Now, there is a vast difference between the tonnage of 10 to 12 tons in an ordinary freight-car with eight wheels passing over rails at a moderate speed, and 15 to 20 tons on the same number of wheels at an increased speed. Since 1874 the Pennsylvania Railroad Company has been steadily increasing the weight of both engines and cars. The duty to which rails are now subjected, I believe, is fully 60 per cent. greater than that before the year 1874. On looking over the paper, we find a number of rails, classed as good-wearing rails, that have for years been subjected to comparatively light tonnage on a wheel-tonnage basis, compared with a great number of rails classed as fast-wearing rails, which in some cases I find have had passing over them nearly twice the number of tons per month, and all on heavier wheel tonnage.

As an illustration, I cite rail No. 937, with the following analysis: carbon, 0·454; silicon, 0·015; phosphorus, 0·145; manganese, 0·726, with only 2 per cent. elongation. In accordance with the deductions and formula this should be a very bad rail, yet on close examination I find that this rail has been subjected to a monthly tonnage of 747,628

tons. If we examine the rail No. 929, which was laid within two miles of rail 937, we find carbon, 0·235; silicon, 0·080; phosphorus, 0·055; manganese, 0·300; elongation, 24 per cent. This rail was subjected to a monthly tonnage of only 381,235 tons, while rail No. 937 was subjected to 96 per cent. more monthly tonnage, and yet rail No. 929 is classed as a good rail. If we assume that 50 per cent. more loaded cars pass east to Philadelphia than pass west from Philadelphia, we find the wheel-tonnage assumes a very important aspect in determining the wearing qualities of rails. Again, the bad rail was in the track 40 months, and only shows a wear of $\frac{6}{100}$ of an inch in vertical section, and has been in the track since the advent of heavier engines and heavier cars, while rail No. 929 has had the advantage of at least 7 years of comparatively light traffic on light-wheel tonnage. The question, which of these two rails has been subjected to the greatest amount of wheel-tonnage, I leave to some one to calculate who has more time to devote to this subject than I have.

In regard to the method proposed by Dr. Dudley to test the rails at the works, I can only say I much prefer the methods suggested by Mr. Sandberg, in his paper read before the Institute at the Lake Superior meeting, with this slight modification, viz.: I would subject a 50 and 52 pound rail to a drop-test of 1800 pounds, falling a distance of 14 feet on the rail, on supports 3 feet apart; for a 54 to 56 pound rail, 16 feet drop; for a 56 to 58 pound rail, 18 feet drop; for a 58 to 60 pound rail, 20 feet drop, and so on in the same ratio. I would also adhere to the test-bar, drawn out from the head of the rail down to one inch square, then placed under a steam-hammer and bent through an angle of 110°, the distance between centres of supports of the bar to be from 10 to 12 inches. Dr. Dudley may think these tests crude; I believe them to be simple, thorough, effective and reliable, and in this I fully concur in the views of Mr. Sandberg.

If Dr. Dudley and the Pennsylvania Railroad authorities believe their deductions are correct, let them have rails made in accordance with the doctor's first formula—phosphorus 0.077, carbon 0·334, silicon 0·060, manganese 0·491—and add to it the less sulphur and copper the better, and, as a matter of course, pay the difference in price involved in the difference in the price of metals; but when Dr. Dudley attempts to formulate a rule to govern the steel-makers, based on his knowledge, I for one decidedly object, and I frankly tell him that

he is opposing all the researches and investigations of the best chemists and metallurgists, both here and abroad.

I have serious and grave doubts if steel made in accordance with his second formula would give a good record in the track. I experimented on this formula in attempting to fill an order. Mr. Sandberg in his paper refers to the filling of an order of 2500 tons on the same formula, and my experience was the same as his. The ingot was a conglomerate mass of honeycombs. It made bad blooms, and I do not believe it made good rails. The rails are now in the tracks of the West Pennsylvania road, and if they do prove to be good rails I shall be very much surprised.

To be continued.

AN ACCOUNT OF EXPERIMENTS MADE BY A BOARD OF UNITED STATES NAVAL ENGINEERS WITH SCREW PROPELLERS OF DIFFERENT MATERIAL AND DIMENSIONS, APPLIED TO THE UNITED STATES FISH COMMISSION'S STEAMER "LOOKOUT," WITH THE HULL COPPERED AND NOT COPPERED.

By Chief-Engineer ISHERWOOD, U. S. Navy.

In the refitting, at the Washington, D. C., navy-yard, of the United States Fish Commission steamer *Lookout*, seven screws were adapted to her, differing in diameter, pitch and fraction of the pitch used, with the view of experimentally ascertaining the best proportions of these for that particular vessel. Two of the screws being of cast iron and the remaining five of cast brass, they furnished an opportunity for observing the comparative effect of these two metals upon the economic efficiency of the screws. Further, as the vessel's bottom was covered with rolled copper during the experiments with two of the screws, while it was not so covered during those with the remaining five screws, but exposed to the water only the wooden surface of a vessel in the usual condition for coppering, the occasion was afforded to determine the effect of such coppering or want of coppering upon the resistance of the hull.

The experiments with these screws were made by a Board of Chief-Engineers of the United States Navy, which reported the data to the Bureau of Steam-Engineering of the Navy Department. The writer has taken these data, corrected and re-arranged them, and from them has made the calculations and deductions that will be found in this paper, none of which are in the Board's report.

The experimental deductions contain two novelties, namely, the effect upon the economic efficiency of screws due to their being constructed of cast brass or of cast iron, and the effect upon the resistance of wooden hulls due to having their bottom coppered or of presenting to the water a wooden surface in the usual state preparatory to receiving the copper; or, in other words, the determination of the comparative resistance to motion in water of hulls having their immersed wetted surface of copper or of planed wood. In the absence of other experiments on these points, the present ones will be of interest, taking care to discriminate that the comparison holds strictly true only for the exact experimental conditions, any variation of which would consequently vary the comparative results; but, although the experiments determine these results quantitatively for only the particular cases on trial, yet they remain qualitatively true for all cases.

Taking the resistance of a hull as composed of the resistance of its immersed wetted surface to the water; and of the resistance of the water to displacement by it, that is to say, the resistance of water to being elevated from the centre of gravity of the greatest immersed transverse section of the hull to whatever height above the water-level it may be forced by the progress of the vessel; there follows that the ratio of these two resistances will vary according to the form or model of the hull. If the hull, of given lineal dimensions, be very sharp, the larger portion of its resistance as a whole, will be composed of its skin resistance or that of its immersed wetted surface to the water, while the remaining smaller portion is composed of the resistance of the water to displacement. And, *vice versa*, if the hull be of very full form, the larger portion of its resistance as a whole will consist of the resistance of the water to displacement, while only the remaining smaller portion is due to the skin or wetted surface resistance. In the case of the *Lookout*, the hull was very sharp, exposing a great extent of immersed wetted surface comparably to its displacement; consequently the difference between the resistances of the two kinds of surface—rolled copper and planed wood—will be proportionally more

marked in her than in the cases of vessels with fuller forms. However hard and smooth a wooden surface may be made out of water, yet when immersed it will become water-soaked and soft, the small exposed fibres of the wood separating, rising and forming a delicate hairy coating, offering much more resistance to water than the original hard plane surface from which they arose.

It is to be regretted that the experiments were so few: they might easily have been extended by cutting off two of the four blades of the screws, by successively reducing their diameters and lengths, etc., which modifications could have been executed at little cost as the screws did not exceed 5 feet in diameter, a size readily handled yet large enough for reliable results; but, as the vessel was not entirely at the command of the Bureau of Steam-Engineering, only such trials were available as could be made without interfering with her other uses.

The experiments do not show any superiority of cast brass over cast iron for the material of the screw; from which may be inferred that, per unit of surface moving at equal speed, the resistance of the water to the cast iron surface is no greater than to the cast brass surface. Also, that the less direct resistance of the thinner cutting or forward edges of the blades of the brass screw was not sufficient to produce a sensible effect on the economic result. The much less cohesive strength of cast iron than of cast brass requires the blades of screws made of the former metal to be much thicker for equal strength than those made of the latter metal; and in the case of the experimental screws, the cast iron blades were about two and a half times thicker than the cast brass ones. Of course, the thicker blades must be attended with more direct resistance than the thinner ones, but the edges, forward and aft, of all blades are so acutely beveled that any difference due to this cause seems to be too slight to affect the result in a marked manner.

Hull.

The *Lookout* was constructed of wood for a steam yacht, and had a light schooner rig. The bottom was uncoppered during the trials with screws *A*, *B*, *C*, *D* and *E*; but it was coppered during the remaining trials with screws *F* and *G*.

The following are the dimensions and proportions of the hull cor-

responding to the draught of water which was maintained without sensible variation during all the trials:

Length on water-line from forward edge of rabbet of stem to after side of the sternpost,	96 feet.
Greatest breadth on water-line,	13 ft. 6 in.
Extreme breadth,	16 feet.
Draught of water from bottom of keel, Forward,	2 ft. 9 in.
Mean,	3 ft. 8 in.
Aft,	4 ft. 7 in.
Depth of keel below lower edge of its rabbet, Forward,	6 in.
Mean,	1 ft. 2 in.
Aft,	1 ft. 10 in.
Mean depth of hull from water-line to lower edge of rabbet of keel,	2 ft. 6 in.
Area of the greatest immersed transverse section,	25·28 sq. ft.
Area of the water-line,	861·44 sq. ft.
Displacement (35 cubic feet per ton),	42·87 tons.
Displacement per inch of draught at water-line,	2·051 tons.
Ratio of length to breadth on water-line.	7·1111
Ratio of the greatest immersed transverse section to its circumscribing parallelogram,	0·7490
Ratio of the water-line to its circumscribing parallelogram,	0·6647
Ratio of the displacement to its circumscribing parallelopipedon,	0 4631

Engine.

The vessel had one vertical, direct-acting, compound, condensing steam engine, with the two cylinders placed side by side and acting on cranks at right angles to each other.

Diameter of the small cylinder,	12 inches.
Diameter of piston rod of the small cylinder,	1½ inches.
Net area of the piston of the small cylinder,	112·2140 sq. in.
Stroke of the piston of the small cylinder,	16 inches.
Space displacement of the piston of the small cylinder, per stroke,	1·0390 cu. ft.
Diameter of the large cylinder,	20 inches.
Diameter of the piston rod of the large cylinder,	1½ inches.
Net area of the piston of the large cylinder,	313·2764 sq. in.

Stroke of the piston of the large cylinder,	16 inches.
Space displacement of the piston of the large cylinder, per stroke,	2·9007 cu. ft.
Ratio of the space displacement of the piston of the large cylinder, per stroke, to that of the piston of the small cylinder,	2·97178

Boiler.

There was one cylindrical boiler, with cylindrical furnaces, and horizontal fire tubes returned over them. A large portion of the steam room was in a steam-chimney or drum placed above the shell and traversed by the chimney, so that the steam in the drum was, in a measure, superheated by the escaping hot gases of combustion.

Diameter of the boiler shell,	7 feet.
Length of the boiler shell,	8 feet.
Diameter of the steam-chimney,	4 ft. 6 in.
Height of the steam-chimney,	5 ft. 6 in.
Number of furnaces,	2
Diameter of the furnaces,	2 ft. 4 in.
Length of the grates,	5 feet.
Area of grate surface,	23½ sq. feet.
Area of water-heating surface, calculated for outside of tubes,	519 sq. feet.
Calorimeter, or aggregate cross area of tubes, for draught,	3·14 sq. feet.
Steam room in boiler shell and in steam-chimney,	94·54 cu. feet.
Square feet of water-heating surface per square foot of grate surface,	22·2429
Square feet of grate surface per square foot of cross area of tubes,	7·4310

Screws.

The screws experimented with were seven in number, and are designated by letters from *A* to *G*, both inclusive. In the tables hereinafter given, the columns headed with these letters contain the data and results of the experiments made with the screws of corresponding letters.

All the screws were of cast brass with the exception of screws *B*

and *G* which were of cast iron and had blades about two and a half times thicker than the brass ones.

All the screws had four blades and a uniform pitch, which varied for the different screws from 7·50 to 9·08 feet. The mean fraction used of the pitch varied also from 0·1994 to 0·5736. The diameter of all the screws, except *A* and *F*, was 5 feet; that of *A* was 4·6458 feet and that of *F* was $4\frac{2}{3}$ feet.

When the vessel was at rest, a small portion of the 5 feet diameter screws protruded above the water; but when the vessel was in motion at the experimental speeds, all the screws were submerged, owing to the dropping of the stern.

The following table shows the principal dimensions of the screws:

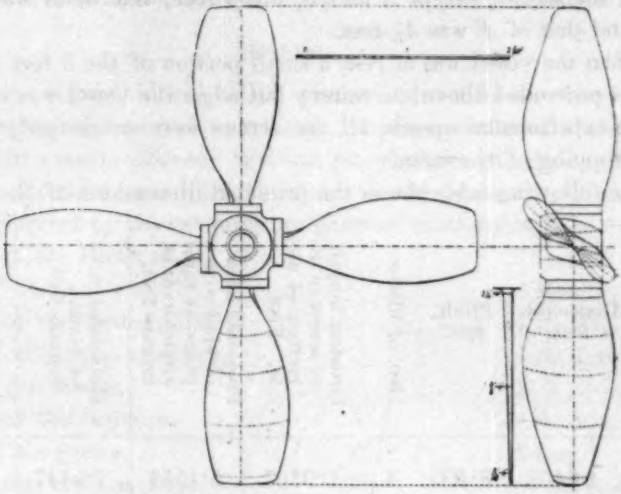
Designation of the screws.	Diameter, feet.	Pitch, feet.	No. of blades.	Greatest length of screw in direction of axis. Feet.	Projected area of blades on a plane at right angles to axis. Square feet.	Helicoidal area of blades. Square feet.	Mean fraction used of the pitch.
A	4·6458	8·400	4	0·9167	5·4551	7·9447	0·3325
B	5·0000	7·500	4	1·1875	10·1693	13·5266	0·5283
C	5·0000	7·500	4	0·5208	5·2457	6·7860	0·2703
D	5·0000	8·625	4	0·4461	3·8697	5·3470	0·1994
E	5·0000	9·000	4	0·8125	6·8975	9·3407	0·3549
F	4·6667	9·000	4	0.8125	5·9659	8·6850	0·3539
G	5·0000	7·900	4	1·2708	11·0098	14·8453	0·5736

The following are detailed descriptions of each screw, wherein the form of the blade, or its outline, is given, together with its thickness and the diameter of the hub, etc.

Screw *A*.

This screw was of cast brass and had a uniform pitch. The blades, when viewed in projection on a plane parallel with the axis, increased

gradually in length from the hub to a radius of 13½ inches, from which their length gradually decreased to the periphery. The hub was square and the blades—which were cast separately—were bolted to it. The thickness of the blades at the fillet of the hub was $1\frac{1}{16}$ inches.

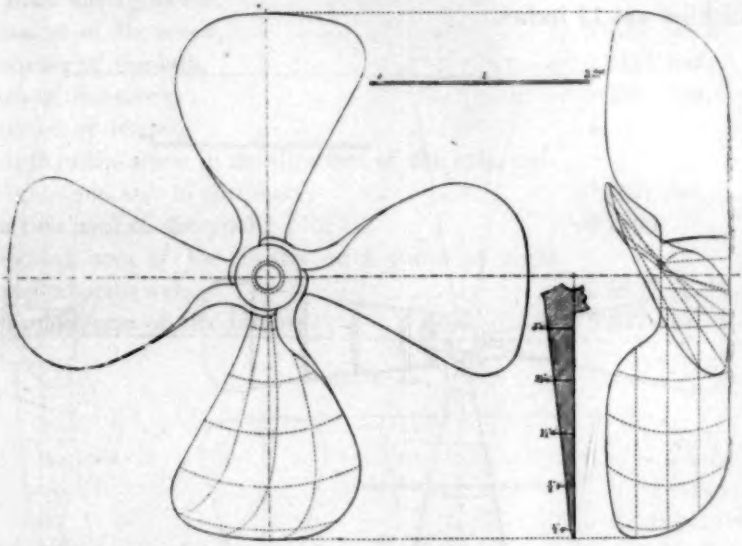


Diameter of the screw,	4·6458 feet.
Diameter of the hub,	0·8333 feet.
Pitch of the screw,	8·4 feet.
Number of blades,	4.
Length of the screw at the hub, in the direction of the axis,	0·5625 foot.
Length of the screw at radius of 13½ inches, in the direction of the axis,	0·9167 foot.
Length of the screw at the periphery, in the direction of the axis,	0·3125 foot.
Mean fraction used of the pitch,	0·3325
Projected area of the blades on a plane at right angles to the axis,	5·4551 sq. ft.
Helicoidal area of the blades,	7·9447 sq. ft.

Screw *B*.

This screw was of cast iron, and had a uniform pitch. The hub and blades were cast together. The blades, when viewed in projec-

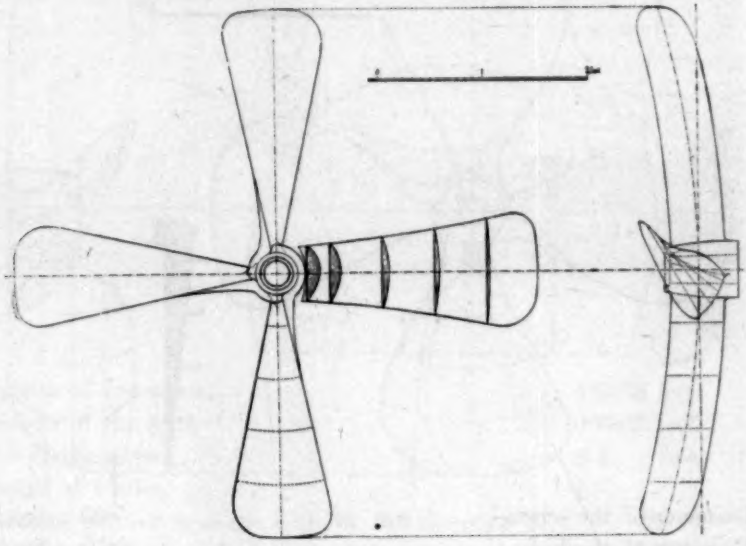
tion on a plane parallel with the axis, had their edges parallel from the hub to a radius of 6½ inches. From this point to a radius of 18 inches the length of the blades gradually but irregularly increased. From the radius of 18 inches to the periphery the length of the blades gradually decreased. The outer corners of the blades were excessively rounded. The hub was cylindrical, and the thickness of the blade at its fillet was 3 inches.



Diameter of the screw,	5·00	feet.
Diameter of the hub,	0·70	foot.
Pitch of the screw,	7·5	feet.
Number of blades,	4.	
Length of the screw at the hub, and to radius of 6½ inches, in the direction of the axis,	0·6667	foot.
Length of the screw at radius of 18 inches, in the direction of the axis,	1·1875	feet.
Length of the screw at the periphery, in the direction of the axis,	0·5080	foot.
Mean fraction used of the pitch,	0·5283	
Projected area of the blades on a plane at right angles to the axis,	10·1693	sq. ft.
Helicoidal area of the blades,	13·5266	sq. ft.

Screw *C.*

This screw was of cast brass, and had a uniform pitch. The hub and blades were cast together. The edges of the blades, when viewed in projection on a plane parallel with the axis, were parallel segments of circles curving backwards, the radius of the forward edge being 9·25 feet. The outer corners of the blades were rounded with easy curves. The hub was cylindrical, and the thickness of the blade at its fillet was $1\frac{3}{4}$ inches.



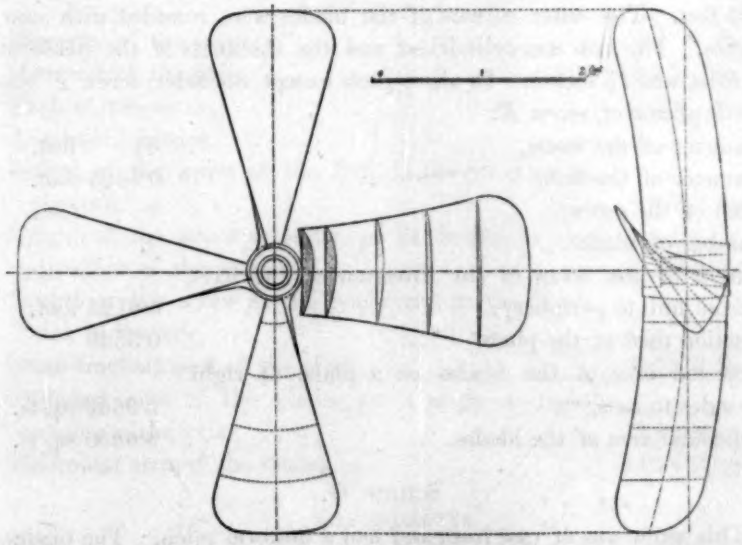
Diameter of the screw,	5·0	feet.
Diameter of the hub,	0·5417	foot.
Pitch of the screw,	7·5	feet.
Number of blades,	4.	
Length of the screw in the direction of the axis, uniform from hub to periphery,	0·5208	foot.
Fraction used of the pitch,	0·2703	
Projected area of the blades on a plane at right angles to the axis,	5·2457	sq. ft.
Helicoidal area of the blades,	6·7860	sq. ft.

Screw *D*.

This screw was of cast brass, and had a uniform pitch. The hub and blades were cast together. The edges of the blades, when viewed in projection on a plane parallel with the axis, were parallel segments of circles curving backwards, the radius of the forward edge being 9·25 feet. The outer corners of the blades were rounded with easy curves. The hub was cylindrical, and the thickness of the blade at its fillet was $1\frac{3}{4}$ inches.

Diameter of the screw,	5·000 feet.
Diameter of the hub,	0·5417 foot.
Pitch of the screw,	8·625 feet.
Number of blades,	4.
Length of the screw in the direction of the axis, uniform from hub to periphery,	0·4461 foot.
Fraction used of the pitch,	0·1994
Projected area of the blades on a plane at right angles to the axis,	3·8697 sq. ft.
Helicoidal area of the blades,	5·3470 sq. ft.

Screw *E*.



This screw was of cast brass, and had a uniform pitch. The hub and blades were cast together. The edges of the blades, when viewed

in projection on a plane parallel with the axis, were parallel segments of circles, the radius of the forward edge being 9·25 feet. The outer corners of the blades were rounded with easy curves. The hub was cylindrical, and the thickness of the blade at its fillet was $1\frac{3}{8}$ inches.

Diameter of the screw,	5· feet.
Diameter of the hub,	0·5625 foot.
Pitch of the screw,	9· feet.
Number of blades,	4.
Length of the screw in the direction of the axis, from hub to periphery,	0·8125 foot.
Fraction used of the pitch,	0·3549
Projected area of the blades on a plane at right angles to axis,	6·8975 sq. ft.
Helicoidal area of the blades,	9·3407 sq. ft.

Screw *F*.

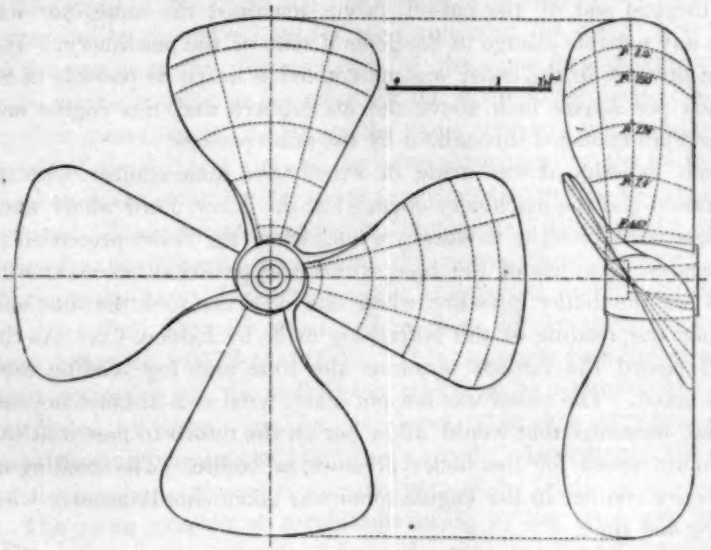
This screw was of cast brass, and had a uniform pitch. The hub and blades were cast together. The edges of the blades, when viewed in projection on a plane parallel with the axis, were parallel segments of circles curving backwards; the radius of the forward edge being 9·25 feet. The outer corners of the blades were rounded with easy curves. The hub was cylindrical, and the thickness of the blade at its fillet was $1\frac{3}{8}$ inches. In all respects except diameter, screw *F* was the duplicate of screw *E*.

Diameter of the screw,	$4\frac{3}{8}$ feet.
Diameter of the hub,	0·5625 foot.
Pitch of the screw,	9· feet.
Number of blades,	4.
Length of the screw in the direction of the axis, from hub to periphery,	0·8125 foot.
Fraction used of the pitch,	0·3539
Projected area of the blades on a plane at right angles to axis,	5·9659 sq. ft.
Helicoidal area of the blades,	8·6850 sq. ft.

Screw *G*.

This screw was of cast iron, and had a uniform pitch. The blades, when viewed in projection on a plane parallel with the axis, increased gradually in length from the hub to a radius of 21 inches, from

which the length gradually decreased to the periphery. The hub was cylindrical, and the thickness of the blade at its fillet was 4 inches.



Diameter of the screw,	5·0	feet.
Diameter of the hub,	0·75	foot.
Pitch of the screw,	7·9	feet.
Number of blades,	4.	
Length of the screw at the hub, in the direction of the axis,	0·8542	foot.
Length of the screw at radius of 21 inches, in the direction of the axis,	1·2708	feet.
Length of the screw at the periphery, in the direction of the axis,	0·8750	foot.
Mean fraction used of the pitch,	0·5736	foot.
Projected area of the blades on a plane at right angles to the axis,	11·0098	sq. ft.
Helicoidal area of the blades,	14·8453	sq. ft.

Experiments.

The experiments were made in the perfectly smooth water of the Potomac River near the city of Washington, over a base of 11⅛ geo-

graphical miles, according to the Coast Survey Chart, extending from marks abreast the wharf at Giesborough Point to marks abreast the wharf at Marshall Hall. During all the experiments, the position of the throttle and of the cut-off valves remained the same, nor was there any sensible change in the general state of the machinery. The steam pressure in the boiler was maintained as nearly as possible at 80 pounds per square inch above the atmosphere, and the engine and boiler were managed throughout by the same persons.

Each experiment was made in exactly the same manner. Steam was raised and the machinery operated at the Navy Yard wharf until the fires were brought to steady action, when the vessel proceeded to the nearest terminus of the base, which she passed at her maximum speed for the boiler pressure, when one observer took the time and another the reading of the taffrail log made by Bliss & Co. As the vessel passed the farthest terminus the time and log reading were again taken. The vessel was not put about until at a distance beyond the last terminus that would allow her on the return to pass it at the maximum speed for the boiler pressure, as before. The reading of the engine counter in the engine room was taken simultaneously with the log and time.

The indicators employed were new, and of Thompson's design; during each run, excepting the first four with screw *A*, there were taken by them six sets of indicator diagrams from each end of each cylinder, the diagrams being equispaced over the time. The indicated pressures in the different experiments are the means of all the diagrams taken. During only the last two runs with screw *A* were there any diagrams taken, and the mean pressure from them has been assumed as the mean pressure for the entire experiment.

During all the runs the vessel's trim and draught of water remained sensibly the same.

The experiments with screws *A*, *B*, *C*, *D* and *E* consisted of six runs over the base with each, three in each direction, and they were made on three different days, a pair of runs being made in immediate succession on each day. The experiments with screws *F* and *G* were made in the same manner, except that they consisted of only four runs over the base, two in each direction, and they were made on two different days, a pair of runs being made in immediate succession on each day.

During experiments *A*, *B*, *C*, *D* and *E* the vessel was not coppered,

the immersed portion of the hull presenting a planed wooden surface to the water. During experiments *F* and *G* this portion was covered with smooth rolled copper.

The speed of the vessel was determined, as stated, by the shore distances and by the taffrail log for each run over the base. The mean of the speeds of the different runs of the same experiment, by the shore marks, is taken as the mean for the entire experiment according to that measurement. And, in the same manner, the mean of the speeds of the different runs of the same experiment by the taffrail log is taken as the mean for the entire experiment according to that measurement. These two measurements differed, and, with a slight irregularity for the different experiments, the mean difference deduced from all the experiments showing, as might be expected, that the indications by the taffrail log were greater than those by the shore marks, the ratio being as 1·0572 to 1·0000. This ratio may be taken as a constant correction for the taffrail log, which, as its determinations were unaffected by current, can be considered, when thus corrected, as the most accurate measure of the vessel's speed. Accordingly, the speed by the log thus corrected is what is given in the following tables.

The mean number of revolutions made by the screw per minute during a run was ascertained from the time and the total number of revolutions given by the counter during that run; and the mean number of revolutions made per minute by the screw during an experiment is the mean of the means of those made during the runs composing that experiment.

(To be continued.)

Volatility of Sulphuric Acid.—Regnault has shown that mercury is volatile even at very low temperatures; but it has been generally thought that sulphuric acid is not volatile at ordinary temperatures. A writer in *Il Progresso* has found that in operating in an atmosphere that had been dried with sulphuric acid, litmus paper was soon discolored and gradually disorganized. This fact may explain the sulphur rays which are often observed in Geissler tubes. It may also be of great importance in chemical analyses, where the vapor of sulphuric acid under certain conditions might change the character of compounds without its presence being suspected.—*Chron. Indust.* C.

PERCUSSION ROCK DRILLS.

By Robert Grimshaw.

Paper read before the Franklin Institute June 15, 1881.

The name "rock drill" is in most cases a misnomer, as the devices used for perforating rock very seldom have a drilling action proper. The French word "*perforatrice*," or perforator, is used with better reason. However, there is little more use in calling attention to this fact, than in stating our use of the word fore-plane is incorrect and the English use correct; the English meaning by fore-plane what we call a jack-plane, which is really the *before* plane.

As ordinarily known, a drill is a device which, by rotation and lengthwise advance, makes a round hole in some solid material, as wood, metal, ivory, etc.

In making holes in rock there are several ways; the oldest, by percussion only, accompanied by intermittent rotation of the drill bit. Very little rock is wrought by drills proper, having steady advance and continuous rotation, and breaking up all the material they remove. There is one type of rock drill which has continuous rotation and continuous advance, and which cuts out an annular channel, leaving in the centre a solid core which may, with care, be removed for inspection. The first and the third of these types—that is, the percussion drill and the ring or rock drill—are most generally in use. I shall in this paper treat of percussion drills only.

The drill proper is the drill bit, although usage sanctions the use of the word drill for the entire machine which guides or which drives the bit. Up to twenty years ago, there were very few drills driven except by hand power, the drill shank being either struck directly by hammer blows or raised by some machine and driven forward by a spring. We will speak of these two methods as the hand drill and the hand-drilling machine systems. So much more work can be done by hand-drilling machines than by hand drilling pure and simple, that the next step was to introduce a machine to imitate hand percussion drilling, with much greater force than by hand direct or by hand power machines. Of course the use of steam and of compressed air in a cylinder, to effect percussion by a drill bit fastened to the piston rod

of the cylinder, naturally suggests itself; the rotation of the drill bit on the back stroke and its advance lengthwise, to correspond with the depth of the cut taken at each stroke, being effected by many mechanical devices. Such a device constitutes the rock-perforating machine; or "rock drill" for short.

As ordinarily constructed, there is a double-acting steam engine, having a piston with a very long head, say 3 inches in diameter, the rod having the drill shank clamped to it direct. The valve motion of this machine, making as it does 300 strokes per minute and upwards, must be effective, simple, durable, compact and, if possible, economical of steam. Ordinarily the distributing valve is a slide valve, or its modification, the reciprocating piston valve; the cylinder having ports from each end of the steam chest in which the valve moves to and fro. The motion of the valve is effected by one of three systems; by lever, or tappets struck by the piston head, or by a boss or its equivalent upon the piston rod; by having the machine duplex; and by fluid transmission without any mechanical connection proper. Some machines have no separate external valve; the piston, or a sleeve thereon, performing the valve office.

The rotary motion is generally given by flutes upon the stem of the piston working in a fluted ratchet nut, although there are other methods; for instance, the stem may be in the back cylinder head and the ratchet nut in the piston head, etc. The forward feed of the drill bit may be by forward motion of the piston in the cylinder, the stroke getting longer each time; or by the stroke remaining the same in length, while changing its place in the cylinder bore; or the cylinder may be advanced. This feed may be either automatic, or by hand; and if automatic, it may be regular and independent of the hardness of the rock being penetrated; or it may be controlled by which this depth of feed may be regulated by hand while the actual work is done automatically; or the forward feed may be determined by the depth of the last cut taken, so that if the drill strikes a hard place and does not cut as deep as when it is working in a soft place, the feed will be lessened. Generally the cylinder heads are cushioned with rubber protected with metal discs, or else there is very great cushion of the driving air or steam, in order to prevent the heads being knocked out by the drill bit coming suddenly into a soft place or a cavity.

As regards the mounting of the machine, this depends upon the

work to be done. If in quarrying or in other surface work, where the holes are all vertical or nearly so, the most common mounting is a tripod having adjustable legs, which are weighted down to keep the machine in place, and are often extensible to allow of more perfect and steady placing. The steam or compressed air is led to the neighborhood of the drill by steam pipes of iron, and from them by flexible hose to the steam chest. If the machine is for gallery work, in which all the holes are horizontal or nearly so, the machine is generally mounted upon a column having thrust screws which jam it against the top and bottom of the heading and hold it firm. In some cases this column is threaded, to allow the machine to be adjusted in height, in others the column is smooth and the machine is clamped at the desired height. In others this column is mounted upon a carriage and has transverse motion by slides upon the carriage frame. In all cases where a column is used, the machine has free adjustment in the horizontal plane and also swings in a vertical plane, so that it has free adjustment after the column is in place, in height, in the vertical angle and in the horizontal angle.

For that class of surface work where it is desirable to channel, or cut a number of holes in a line close together, there is an arrangement permitting this to be done. In some cases, for tunnel work, several drills are mounted upon a carriage and all are given lengthwise advance at the same time by this carriage, although each one may cut at any desired angle in the vertical or horizontal plane. For those situations where it is necessary to cut down close to the floor, a special type of frame must be used, and in some cases a special type of machine. I show on the screen several types of drills and several styles of mounting them. Some of the illustrations are from working drawings of the makers.

It can be said about the tappet system of working a valve, that, while giving a positive motion to the valve, there is liability to and there always has been trouble from breakage of the tappets and their slight working parts, which are struck so many times a minute with such great force. This trouble is increased in frosty weather and this brings the repair bill up very high. Some opponents of tappets think that because the piston has to strike the tappet on the working stroke, the full force of the blow is not delivered upon the rock; but it seems to me that this is but a trivial matter.

The duplex system is not in use in this country. Because of the

very rough usage that the rock perforating machine has to undergo, it is well that there be as few exposed working portions as possible.

In reference to automatic feed; this is not generally applied to small machines, because these being used underground, light weight and simplicity is desirable. Where automatic feed is used, but one man is needed to work the machine.

In reference to steam or air cushion, those who oppose it do so on the ground that the motive fluid must be introduced by this means, in front of the advancing piston; so that, although the head is preserved from being knocked out, the blow is lessened in force and deepness.

Rotation is for two purposes; first to make a perfectly round hole, and second to make the bit work not only by mashing away the rock in front of it, but also by wedging off a portion of rock between the new cut and the last old one. To this a third reason may be added, that it preserves the edge of the bit, thereby lessening the wear and saving cost of sharpening.

The bits are of various shapes; the most common being in the form of an X. For loose, seamy rock, a Z shape is found to answer well.

The adjustability of the tripod may seem a very little thing, but it means a great deal. A machine properly mounted will work where two men cannot work with hammers upon the face of the cut. In a seam only two feet thick, like that at Port Henry, New York, the machine has to work nearly flat, with the legs spread apart.

By employing a lateral arm clamped to the column, the drill may be moved in or out on the arm, and the arm moved up or down or moved around, thus commanding a large portion of the breast without moving the column. The column should thrust against short pieces of timber at top and bottom. Columns over 8 feet long are apt to give give trouble from vibration. For a very large work, as tunnels, the column may be used to work out a drift of considerable size in extension of the line of the roof, and the tripods used to take up the bottom. This has the advantage of dividing the drilling ground and enabling more men to be used.

The disadvantage of the carriage is that the ground must be cleared before the carriage can be run up to the heading.

As regards the size of drills to use: 5-inch is used for submarine work, mounted upon a scow or frame; for deep heavy tunneling,

mounted upon a carriage; and for deep rock cutting, mounted upon a tripod. This size will drill from 1 to 40 feet deep and from 2 to 6 inches in diameter.

The 4-inch is for tunneling, heavy straight grading and quarry work, and where 12 to 20 feet holes, 2 to 4 inches in diameter, are to be made in very hard rock.

The 3½-inch and the 3-inch are the most used; being found in quarries, railroad tunnels, grading, sewers and mining; the 3½-inch drilling a 12 feet hole, 1½ to 2½ inches in diameter, and the 3-inch drilling an 8-feet hole, 1 to 2 inches in diameter. Below this size there are the 2¾ and 2½-inch drills.

For horizontal drilling the capacities are about ¼ less. As regards the capacity of these drills, I annex some figures showing what they will do in various kinds of rock, and in some cases showing the rate of hand work in the same rock.

There is some work, such as cutting slate, granite and marble, where blasting cannot be used for fear of breaking the stone, and in this case 2-inch holes are drilled in a row, two inches apart, and the connection broken down by throwing out the rotation gear, and working out the stone between the holes with a drill bit having a flat point.

As far as possible, the drilling machine should be light, compact, strong, portable, quickly set up and moved, simple in construction, readily repaired at the shaft-head, economical of steam or compressed air, rapid in motion, free from trouble in freezing up where compressed air is used. If possible, the drill should be withdrawn automatically when desired. The machine should strike the blow variably according to the rock being entered. It is desirable that the hole be churned out by the machine itself, and that the bit shank may be quickly attached to the piston rod. Some like self-feed, some do not. The machine must be readily taken apart and kept clean and in working order. It must drill deep at one setting up. The heads should not knock out, and the tappets, if there be any, should not break. The piston rod should be large.

In the Johnson machine there is self-feed and steam or air pull-back. The whole cylinder may be slipped through the body clamp to or from the rock, thus adding to the length of the feed.

In the Bryer drill there are but two working parts—the piston and the rotation bar, the piston being its own valve, regulating the admis-

sion, cut-off and exhausts by annular grooves in the cylinder and piston. It is steam-cushioned at each end.

Place or works.	Diameter of piston.	Size of hole.	Material passed through.	Feet drilled per hour, day or month.	Cost per ft. (Drill hole.)		Sav'g over hand dr'g.
		in.			Power.	Hand	Per cent.
Yellow Jacket Silver Mining Co.	.....	...	Conglom., Feldspar, Porph. and Quartz.	1160 ft. mo.	$1-.02⅔	.....	50
Overman Mining Co.	.....	...	Hard blasting rock & Quartz.	2000 ft. mo.	.18¾	.....	33⅓
Musconetcong Tunnel	5 in.	...	.....	25 to 40 ft. per day.	.....	.....	.....
Sierra Nevada Mining Co.	.....	...	Porphyry.	.....	.32	.....	50
Eagle Harbor and Ahnepee.	5 in.	...	Trap and Limestone.	5387 } working 4491 } seasons '75, '76, '77.	.....	.....	.....
Ausable Forks, N. Y.	.....	1½	Feldsp'r ore and Feldspathic rock.	.....	.....	.....	50
Canada Pacific R. R., Manitoba.	5 in.	...	....	207 ft. 7 in. in 3 days.	.....	.....	.....
Millstone Point, Conn.	.....	2	.....	7 ft. 8 in. per hr.	.....	.....	.....
Iron Mountain Co., Mo.	.....	3¼	Iron ore, Porph. and Limestone.	45 ft. per d. of 10 hours.	.23⅔	.83	66
Knoxville, Tenn.	.....	...	Marble.	140 ft. P. D. 23 " " hand 3 men.	.....	.....	.....
Chicago & Colorado Mining Co.	4 in.	...	.....	22 in., 1 man 10 hours. 5 ft. in 30 min., P. Drill.	.....	.....	.....
Paxton Furn'e, Pa.	.....	2¼	Limestone.	45 ft. P. Dr. 8 " hand.	.12	.50	75
Leesport.	3¼ in	...	Limestone.	40 ft., P. D. 3 ft. 4 in. by hand, 1 man	.....	.....	.....
Denver & South Park R. R.	3½ in	...	Granite, Iron rock.	80 ft in 10 hours. 9 ft. with 2 hands.	.....	.....	.....
Wakefield Marble Quarries.	3 in.	2	Marble.	65 ft. per day.	.....	.....	.....
Diamond Hill Granite Co.	3 in.	...	Granite.	40 " " "	.....	.....	.....
Steelton, Pa.	.....	...	Limestone.	100 " " "	.04½	.61	.....
Georgetown, Col.	.....	...	.....	90 " " "	.....	.....	.....

In the Rand drill the tappets are moved by the piston instead of by projections upon the piston rod. The feed in the machine is by hand, a square-threaded screw carrying the cylinder along on its bed-plate, which latter has a vertical pivot.

In the Burleigh tappet drill there is a trough with ways on each side, in which the cylinder slides. The screw feed is automatic. The piston rod, which has a double annular cam and spiral grooves, controls the valve, effects the feed, and causes bit rotation. It is sometimes mounted in gangs in a frame or carriage, two on a horizontal bar across the top and two at the bottom, each having adjustability in these planes.

The single drill is mounted on a threaded telescopic iron column, having a single sharp point at the bottom and an iron thrust claw at the top. This column may also be used horizontally for shaft work.

The new Sergeant drill, made by the Ingersoll Rock Drill Co., was described at length, and illustrated, by Mr. F. L. Miller, in a paper before the Engineers' Club of Philadelphia, September, 1880. I show upon the screen the working drawings of the latest form. The principal peculiarities are in the valve and the ports and passages. The valve is a cylinder, having flanges somewhat smaller than the cylindrical steam chest, in which the valve slides upon a central bolt, which serves to hold the chest heads together. The chest heads have rubber cushions. The piston is of great length, and has a cavity as long as the piston stroke, so that it will always register with the ports and allow the steam exhaust from the valve to be exhausted therein. We will suppose the valve at one end of the stroke and steam let into the chest; the steam will pass to the cavity between the flanges, the steam being exhausted at the other end there will be no opposition to the valve moving towards that end, by reason of the steam rushing past the flange nearest the valve cylinder end. The cavity in the main piston prevents the valve being shifted until the main piston is nearly at the end of its stroke, when it will uncover the exhaust port of the valve cylinder in which the steam is confined, the steam passing into the upper port then to the opposite end of the chest, then by the lower port to the cavity in the piston. Rotation is by a fluted bar and nut; and feed by hand.

RADIO-DYNAMICS.

By Pliny Earle Chase, LL.D.

Abstract of lectures delivered before the Franklin Institute, March 10 and 17, 1881.

Your committee have invited me to lecture upon some of the results of investigations in which I have been specially engaged. My subject is given, in one part of the announcement, as astronomy; in another, as the music of the spheres. The former title is so far appropriate as it designates the source from which the greater part of my discoveries have been derived; the latter, as indicating the universal harmonies which are manifested, both by atoms and by stars, by microscopic and macrocosmic spheres alike, and which are, as I shall try to show you, the necessary results of the plan which has established the stability of the physical universe.

It will be impossible, in two lectures, to do more than glance at a few of the instances of prevailing rhythm, but I think you will find those which I have time to bring before you quite sufficient to serve as the solid groundwork of a science which is both the oldest and the newest of all sciences—the science of photo-dynamics or radio-dynamics. I call it the oldest, because we are told in Genesis that the first act of the Creator, in educing order out of chaos, was the command: "Let there be light;" the newest, because its right to recognition is as yet but sparingly and somewhat hesitatingly accepted, and because nearly all the materials, with which it has to deal in its systematic coördination, have been collected within the last quarter of a century.

The scientific spirit strives always to ascend from the special to the general; from multiplicity to unity. The Greek philosophers looked, in turns, to each of their four elements—earth, air, fire and water—as the basis of all things. Newton, in his "Principia," demonstrated many propositions which are applicable in all fields of physical investigation, but he used them only for explaining the motions of the various members of the solar system. He spoke, however, of an "æthereal spirit," as a possible medium in universal gravitation, but without giving any hint of believing that any of its properties were within the reach of physical research. Franklin's experiments in electricity furnished a foundation for electro-dynamics, and led to a

belief, which is still widely held, that in the various forms of electrical manifestation the clue to all physical activity is to be found. Mayer, Joule and their collaborators opened the gates of that fairy-land of science which Tyndall has so admirably described in his "Heat as a Mode of Motion," and there are many who now believe that all material phenomena are susceptible of an explanation by thermo-dynamic laws.

The theory of the "correlation of forces," which teaches that light, heat, electricity, magnetism and chemical affinity are all forms of a single energy, and that they all may be interchangeably converted, provided the proper conditions are observed, may be thought to imply that neither of the correlated sciences is entitled to any precedence over the others, but that each of them becomes tributary to the general science of universal physics, so far as it develops laws which are of universal application.

Sir John Herschel appears to have been the first investigator who ever proposed any numerical estimate of the energy of light. It is a well-known proposition that the velocity of wave propagation, in elastic media, varies directly as the square root of the elasticity and inversely as the square root of the density. He accordingly stated, in his "Familiar Lectures on Scientific Subjects" (pp. 281-3), that the elastic force of the air, in its resistance to compression, would require to be increased "*in proportion to the inertia of its molecules*" more than 1,000,000,000,000-fold, to admit of the propagation of a wave with the velocity of light, and that this enormous physical force is perpetually exerted at every point through all the immensity of space. He also said (p. 218): "It must be remembered that it is LIGHT, *and the free communication of it from the remotest region of the universe*, which alone can give and does give us the assurance of a uniform and all-pervading energy."

In the eloquent extract which is quoted by Tyndall (*op. cit.*, 4th ed., section 707), Herschel had previously stated that "the sun's rays are the ultimate source of almost every motion which takes place on the surface of the earth." Tyndall, with equal eloquence (*Ibid.*, section 724), describes the flux of power which "rolls in music through the ages," and shows that all "the integrated energies of our world . . are generated by a portion of the sun's energy which does not amount to $\frac{1}{2300000000}$ of the whole."

These extracts seem to furnish a sufficient reason for looking upon

solar radiation as the basis of all terrestrial physics, and upon radio-dynamics, or the science which refers all physical activity to centres of energy, as the universal physical science. Gravitation, cohesion and chemical affinity are directly concerned only with centripetal phases of force; inertia, in orbital and explosive motions, introduces a kind of centrifugal action; heat, properly speaking, seems to be wholly centrifugal, for the approach of particles when heat is radiated can hardly be attributed to thermo-dynamic action; electricity and magnetism, as positive and negative, boreal and austral, are both centripetal and centrifugal; light, according to the undulatory hypothesis, also represents both phases of activity, in the alternate contractions and expansions of wave propagation, as well as in the phenomena of radiation, refraction, reflection and coloration.

Electricity and light have been connected, and to some extent identified, by means of investigations which were begun by Weber and Kohlrausch, in Germany, and continued by Thomson, Maxwell, Ayrton and Perry, in England. As a result of those investigations, it has been found that electro-magnetism is related to electro-statics, somewhat as momentum to mass, the electro-magnetic unit being equivalent to the electro-static unit multiplied by the velocity of light.

Maxwell, accordingly, regarded light as an electro-magnetic phenomenon. It seems to me more logical to regard electro-magnetism as a luminous or radial phenomenon, for the following reasons:

1. Because the velocity of light is only one factor of electro-magnetism, but it is the important factor which constitutes it a force.

2. Because we have no evidence of electro-magnetic action in space, while we have much evidence of the action of light.

3. Because the eminent practical observers, who have studied the phenomena of terrestrial magnetism most carefully, have concluded that there is no specific magnetism in the sun and moon to influence the terrestrial magnetism through induction.

4. Because the mass-factor, which constitutes an important though subordinate element in all thermal, chemical, electrical and magnetic phenomena, is mainly, at least so far as it appears most obviously in those phenomena, a terrestrial factor.

5. Because it is better to designate the solar radiations by a name which will be universally recognized as appropriate, than by a name which has been generally applied only to local phenomena.

A still stronger and perhaps conclusive reason for regarding photo-dynamics as a special and principal department of radio-dynamics, is the fact that the velocity of light, as I propose to show you, is an important factor of gravitating, as well as of electro-magnetic action. In studying the phenomena of gravitation, there is no necessity for introducing any other elements than those of simple *vis viva*, mass and the square of the velocity. If the limit of efficient velocity can be shown to be the velocity of light in both departments, the law of parsimony would exclude the electro-static unit, unless it can be shown that it is a necessary element of mass. This has never yet been done. If the necessity should be demonstrated hereafter, it is more likely that it will be found to depend upon some modification of the fundamental velocity of light than upon any independent activity which can be regarded as purely electrical.

The chief postulate of photo-dynamics may be stated as follows: *All physical phenomena are due to an Omnipresent Power, acting in ways which may be represented by harmonic or cyclical undulations in an elastic medium.*

The Omnipresent Power is scientifically required by the law of harmony; the harmonic or cyclical undulations, by the law of permanence or stability; the representative elastic medium, by the law of equal and opposite action and reaction. All questions as to the reality or nature of the supposed medium are of minor importance. Although my investigations have strengthened my own belief in the reality of an all-pervading æther, we are only required to recognize the existence of phenomena which involve such actions, and can be explained by such laws, as have been deduced from the motions of the atmosphere and other elastic fluids.

The following well-known laws have an important bearing upon photo-dynamics:

1. Cyclical activities may often be accurately represented by formulas which introduce mean or average velocities and mean *vis viva*. This is the foundation of Maxwell's theory of the equality of mean *vis viva* in the molecular movements of different gases at equal temperatures, and of Pfaundler's discovery that in estimating the heat of dissociation, the mean should be taken between the temperatures of incipient and of complete dissociation.

2. The projectile force, which produces flight or cyclical motion against any central acceleration or retardation, is equivalent to the

mean acceleration or retardation multiplied by one-half the time of flight or cyclical motion.

3. The velocities of wave motion in elastic fluids, and of cosmical and molecular orbital motion, can all be expressed by a common formula.

4. Every periodic vibrating or orbital motion can be regarded as the sum of a certain number of pendulum vibrations.

5. Mean *vis viva* may be represented by the *vis viva* of centres of oscillation.

6. The distance of the centre of oscillation from the centre of relative stability is at two-thirds of the length of a linear pendulum, or at the square root of four-tenths of radius in a rotating sphere.

7. The acceleration of any force, which is uniformly diffused from or towards a given centre, varies inversely as the square of the distance from the centre.

8. Times of revolution, under the action of such forces, vary as the three halves power of the distance; distances vary as the two-thirds power of the time.

9. Centres of inertia, or nodes, in a vibrating elastic medium, tend to produce harmonic nodes.

10. The force of planetary projection should be referred to perihelion; the force of incipient subsidence, to aphelion.

11. The mutual inter-actions of cosmical, molecular or atomic bodies are proportioned to the respective masses; actions which are considered with reference to a single active centre vary directly as the mass and inversely as the square of the distance.

12. In elastic atmospheres the densities decrease in geometrical progression, as the height above the surface increases in arithmetical progression.

13. Living force, or *vis viva*, is proportional to the product of mass by the square of the velocity.

14. The distance of projection against uniform resistance is proportioned to the living force.

15. In synchronous orbits, the mean velocity of rectlinear oscillation is to the velocity of circular orbital oscillation as twice the diameter is to the circumference.

16. In a condensing nebula, the velocity of circular orbital revolution is acquired by subsidence, from a state of rest, through one-half of radius.

The following additional propositions may be readily deduced from the foregoing.

17. The acceleration or retardation of a centripetal force varies as the fourth power of the velocity of orbital revolution.

18. In cyclical motions, the resultant of all internal forces must be in equilibrium with the resultant of all external forces, at the expiration of each half cycle.

19. The modulus of cyclical motion is equal to the product of acceleration by the square of the time of a half cycle.

20. The sum of all external forces may, therefore, be represented (2) by a velocity which is equivalent to the mean or resultant internal force acting for one-half of the cyclical time.

21. At the extremity of a linear pendulum, the influence of a central force on the centre of oscillation is nine times as great as on the centre of suspension.

22. The limiting *vis viva* of wave propagation is five-ninths of the mean *vis viva* of the oscillating particles.

23. In condensing nebulæ, rupturing forces which are due to central subsidence may be represented by fractions in which the denominator is one greater than the numerator.

24. In synchronous rotation and revolution, the nucleal radius varies as the three-fourths power of the limiting atmospheric radius.

25. The variation in mean *vis viva* of gaseous volume is to the variation in *vis viva* of uniform velocity as 1 is to 1·4232.

26. The mean thermal and mechanical influences of the sun must be in equilibrium.

27. The collisions of particles, in subsiding towards a centre of force, tend to form belts at the centre of linear oscillation.

28. The limiting velocity between tendencies to aggregation and tendencies to dissociation is to the velocity in a circular orbit as the ratio of the circumference of a circle to its diameter is to the square root of two.

29. In explosive, as well as in cyclical motions, equilibrium must be established between internal and external forces.

30. Apsidal and mean planetary positions must also be controlled by like tendencies to equilibrium.

31. Undulations in an elastic medium maintain the primitive velocity which is due to their place of origination.

32. When two or more cyclical motions are combined, they must all be modified by the tendency to conservation of areas.

33. In expanding or condensing nebulæ, the conservation of areas maintains a constant value for the modulus of rotation.

34. Instantaneous action between different masses or particles by mere material intervention is impossible.

35. In synchronous motions about different centres, the mean distances from the centres of motion vary as the cube root of the masses or other controlling forces.

36. Constant velocities, in a homogeneous elastic medium, represent constant living forces.

In applying these general principles, we must expect to meet with perturbations, arising from the adjustment of opposing tendencies. If the problem of three bodies is so difficult, in astronomy, as to defy all efforts at satisfactory solution, the attempt to grapple with all the intricacies of elastic interaction may also defy the ordinary methods of mathematical analysis. And yet, by paying proper regard to mean values, it is possible, through very brief and simple processes, to get approximate determinations of important astronomical and physical constants, in which the error is less than in the ordinary approximations which require long, tedious and intricate calculations.

Lockyer's late spectroscopic researches have awakened a new interest in the old theory, that all chemical elements are merely different forms of condensed æther, and that the æther itself is only a universal atmosphere. Taking this theory as a provisional hypothesis, there can be little question that hydrogen is the element which resembles the æther most closely, and which may, therefore, be regarded either as the first step in elementary condensation, or as the transmitter of primordial undulation. It is the lightest of all known substances; it is hyper-elastic, being the only gas in which the elasticity increases faster than the condensation; it is always present in solar explosions, if the evidence of the spectroscope is trustworthy; the height to which it is thrown, and the rapidity of its diffusion, in these explosions, indicate a force and velocity which can be best explained by photo-dynamic influence; there are many reasons for believing that it is the outer envelope of the sun; and it presents many features of peculiar interest in connection with Lockyer's basic lines, which furnish simple harmonic indications of great significance.

In order to illustrate some of the properties of hydrogen I have

prepared a pipe and some some soap suds, for blowing bubbles, and by making a connection with the receivers I am able to inflate the bubbles with a mixture of oxygen and hydrogen.

You see with what rapidity the little balloons mount to the ceiling. I touch them with a candle and you are startled by their explosion. Doubtless most of you have seen the experiment before, and have learned that when the gaseous particles rush together, after the explosion, they are joined chemically so as to form water; but I think none of you have ever dreamed of any possible bond of union between the explosion and satellite revolution, or of weighing the bubbles in a scale with the sun. That there is such a bond, and that the sun can be thus weighed, I will try to show you.

Tyndall has told us (*op. cit.*, section 181) that the force of explosion, in one pound of hydrogen uniting with eight pounds of oxygen, is "equivalent in energy to the descent of a ton weight down a precipice 22,320 feet high"; it would, therefore, be sufficient to lift a ton to the top of such a precipice. If it were all concentrated upon the hydrogen alone, that gas would be driven entirely beyond the reach of the earth's attraction; but it carries with it the eight pounds of oxygen, and, notwithstanding this ninefold burden, if there were no resistance from the air, the watery vapor would be lifted more than two thousand miles before it would begin to fall to the earth again. The velocity with which it starts is more than five miles per second, or nearly one per cent. more than the velocity with which a satellite would revolve at the surface of the earth.

You have already learned that circular orbital velocity is acquired (16) by falling through half the distance to the centre; therefore the combining energy of water is more than sufficient, if it were not for the resistances of the air and of friction, to keep it in perpetual revolution. Those resistances do not destroy the motion; they merely change it into heat, electricity, magnetism, chemical affinity, molecular vibration, or some other form of cyclical oscillation.

Do you think that these harmonies are merely accidental, or that they can be so regarded with any reasonable probability? In order to remove any possible doubts upon the question, I will ask you to follow me still further.

According to the kinetic theory of gases, the particles are in perpetual motion, and the gaseous elasticity is owing to the force of

repeated collisions. You may accept or reject the theory as you please, but all the known properties of elastic fluids are such as they would be if the theory were true. It may, therefore, be safely assumed as a guide to new investigations. In subsidence from the satellite orbit of watery vapor, when the orbit velocity is increased twofold, the gravitating acceleration (17) is increased sixteenfold. Now, if we multiply this increased acceleration by the molecular velocity of hydrogen, the product is the same as if we multiply the original acceleration by the orbital velocity of the earth, so that the explosion of our soap bubbles furnishes us with all the data which are needed for weighing the sun and measuring its distance.

In order to make our approximations as close as possible, it is desirable to check, or confirm them, by finding some other harmony of a similar character. We look naturally, in the first place, to hydrogen's companion in its plunge down the mighty precipice, and we find that oxygen stands in a still closer relation to earth's velocity of rotation than that in which hydrogen stands to earth's orbital velocity. If we divide earth's equatorial circumference by the number of seconds in a sidereal day, we find that its rotating velocity is 1525·7 feet per second, which is precisely the velocity of oxygen, according to the experiments of Clausius, at the temperature of 4·8°C. This is within the limits of possible uncertainty of the temperature of water at its greatest density, the commonly accepted temperature being 4°C.

This harmony may be extended so as to include all gases through Maxwell's law of equality of gaseous *vis viva*.

Substituting atomic weight for gravitating acceleration, and remembering that orbital *vis viva*, in equal volumes, is proportioned to the gravitating acceleration, the mean velocity of hydrogen at 4·8°C. can be readily deduced from the mean velocity, at the same temperature, of any other gas of known atomic weight. If we adopt Regnault's value for the atomic weight of oxygen, 15·96, the mean distance of the sun is 92,769,000 miles; its mass 331,595; and the velocity of light is 186,400 miles per second. These results, though so simply deduced, are fully as trustworthy as any that astronomers have yet reached, after thousands of years of patient observation and tedious calculation.

MODIFICATION OF WHEATSTONE'S MICROPHONE AND ITS APPLICABILITY TO RADIOPHONIC RESEARCHES.

By ALEXANDER GRAHAM BELL.

A paper read before the Philosophical Society of Washington, D.C., June, 11, 1881.

In August, 1880, I directed attention to the fact that thin disks or diaphragms of various materials become sonorous when exposed to the action of an intermittent beam of sunlight, and I stated my belief that the sounds were due to molecular disturbances produced in the substance composing the diaphragm.* Shortly afterwards Lord Raleigh undertook a mathematical investigation of the subject, and came to the conclusion that the audible effects were caused by the bending of the plates under unequal heating.† This explanation has recently been called in question by Mr. Preece,‡ who has expressed the opinion that although vibrations may be produced in the disks by the action of the intermittent beam, such vibrations are not the cause of the sonorous effects observed. According to him, the ærial disturbances that produce the sound arise spontaneously in the air itself by sudden expansion due to heat communicated from the diaphragm—every increase of heat giving rise to a fresh pulse of air. Mr. Preece was led to discard the theoretical explanation of Lord Raleigh on account of the failure of experiments undertaken to test the theory. He was thus forced—by the supposed insufficiency of the explanation—to seek in some other direction the cause of the phenomenon observed, and as a consequence he adopted the ingenious hypothesis alluded to above. But the experiments which had proved unsuccessful in the hands of Mr. Preece were perfectly successful when repeated in America under better conditions of experiment, and the supposed necessity for another hypothesis at once vanished. I have shown in a recent paper read before the National Academy of Science,‖ that audible sounds result from the expansion and contraction of the material exposed to the beam; and that a real to and fro vibration of the dia-

* Amer. Asso. for Advancement of Science, Aug. 27, 1880.

† Nature, vol. xxiii, p. 274.

‡ Roy. Soc., Mar. 10, 1881.

‖ April 21, 1881.

phragm occurs, capable of producing sonorous effects. It has occurred to me that Mr. Preece's failure to detect with a delicate microphone the sonorous vibrations that were so easily observed in our experiments might be explained upon the supposition that he had employed the ordinary form of Hughes' microphone, shown in Fig. 1, and that the vibrating area was confined to the central portion of the disk. Under such circumstances it might easily happen that both the supports (*A B*) of the microphone might touch portions of the diaphragm which were practically at rest. It would of course be interesting to ascertain whether any such localization of the vibration as that supposed really occurred, and I have great pleasure in showing to you to-night the apparatus by means of which this point has been investigated (see Fig. 2).

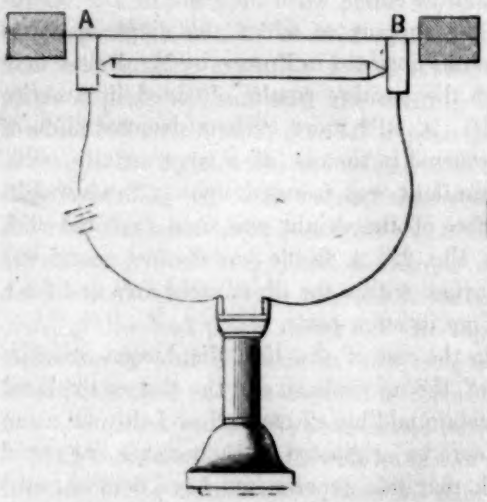


Fig. 1.

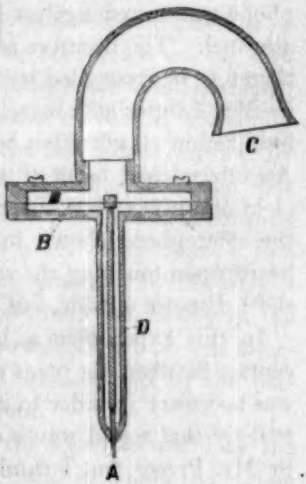


Fig. 2.

The instrument is a modification of the form of microphone devised in 1827, by the late Sir Charles Wheatstone, and it consists essentially of a stiff wire (*A*), one end of which is rigidly attached to the centre of a metallic diaphragm (*B*). In Wheatstone's original arrangement the diaphragm was placed directly against the ear, and the free extremity of the wire was rested against some sounding body—like a watch. In the present arrangement the diaphragm is clamped at the circumference like a telephone-diaphragm, and the sounds are con-

veyed to the ear through a rubber hearing tube (*C*). The wire passes through the perforated handle (*D*), and is exposed only at the extremity. When the wire (*A*) was rested against the centre of a diaphragm upon which was focussed an intermittent beam of sunlight a clear musical tone was perceived by applying the ear to the hearing tube (*C*). The surface of the diaphragm was then explored with the point of the microphone, and sounds were obtained in all parts of the illuminated area and in the corresponding area on the other side of the diaphragm. Outside of this area on both sides of the diaphragm the sounds became weaker and weaker until at a certain distance from the centre they could no longer be perceived.

At the points where one would naturally place the supports of a Hughes microphone (see Fig. 1), no sound was observed. We were also unable to detect any audible effects when the point of the microphone was rested against the support to which the diaphragm was attached. The negative results obtained in Europe by Mr. Preece may therefore be reconciled with the positive results obtained in America by Mr. Tainter and myself. A still more curious demonstration of localization of vibration occurred in the case of a large metallic mass. An intermittent beam of sunlight was focussed upon a brass weight (1 kilogram), and the surface of the weight was then explored with the microphone shown in Fig. 2. A feeble but distinct sound was heard upon touching the surface within the illuminated area and for a short distance outside, but not in other parts.

In this experiment as in the case of the thin diaphragm absolute contact between the point of the microphone and the surface explored was necessary in order to obtain audible effects. Now I do not mean to deny that sound waves may be originated in the manner suggested by Mr. Preece, but I think that our experiments have demonstrated that the kind of action described by Lord Raleigh actually occurs, and that it is sufficient to account for the audible effects observed.

STORED-UP ELECTRICITY: FAURE'S SECONDARY BATTERY.

A few weeks since the scientific world in Paris was deeply interested by a paper read before the Société d'Encouragement de l'Industrie Nationale by M. Reynier, upon a new form of battery invented

by M. Camille Faure, who, following in the steps of M. Gaston Planté, had succeeded—according to M. Reynier—in devising a battery in which forty times as much electric energy may be stored up as could be done by the Planté pile, the result being that a large amount of force easily convertible into mechanical work, or adaptable for electric lighting, could be stored up in small and easily portable cells, could be transported from place to place, delivered from house to house, and that the great problem of a domestic electric lighting and power supply was thus solved.

M. Faure's secondary battery is an application of a new discovery to the very beautiful and well-known secondary pile of M. Gaston Planté, which our readers will remember consists of two plates of sheet lead separated from one another and immersed in a glass jar of diluted sulphuric acid; if these two plates are connected for a time with the terminals of a source of electricity such a dynamo-electric machine or a voltaic battery, oxidation and deoxidation take place on the two plates respectively, and after the exciting battery has been removed, the lead cell continues to give off a polarization current of electricity as long as the deoxidation and oxidation of the lead plates continue by their returning to their normal condition. It was from the first observed that secondary piles of this construction produced better results after having been charged and discharged a great many times, a fact due no doubt partly to the increase of surface produced by the roughening of the lead plates under the decomposition, but chiefly to the formation of lead peroxide in increasing quantities, which was alternately deposited and decomposed as the cell was charged by the battery and discharged by the polarization current.

M. Faure (whose name is well known in the scientific world as the inventor of the battery which bears his name, and in which the carbon element in a Bunsen's cell is made in the form of a bottle which contains the nitric acid) has recently introduced an important improvement to the Planté cell, by which its capacity is largely increased, so that an apparatus constructed upon his principle is capable of producing a much greater current than that given off by a secondary battery of the old construction and of the same size. As the capacity of a secondary battery, other things being equal, is due to the thickness of the layer of lead peroxide formed upon one of the lead plates, M. Faure conceived the idea of coating each of the plates with a thickness of red lead maintained in its place by a sheet of felt attached to the plate by means of lead

rivets. Both plates having been similarly treated they are rolled together into a spiral, the felt performing the two-fold duty of separating the plates and holding on the coating of read lead. This couple is then immersed in acid contained in a cylindrical cell of lead and connected by its electrode to the poles of a dynamo-electric machine or voltaic battery, and after having been charged and discharged two or three times, the red lead coatings of the plates are found to have undergone a change, the one having been entirely transformed into peroxide of lead, while the other has been reduced to the metallic state, and as this result must be due to the oxidation of the red lead on the one plate and the deoxidation of that on the other, it would appear that what may be called the storage capacity of the apparatus depends upon the quantity of red lead carried by the plates.

There can be no doubt that in this way electricity may be "bottled up" and "stored" to an almost unlimited extent, and in this bottled-up condition can be carried in reservoirs to a distance, there to be utilized until it is exhausted, just as a reservoir of compressed air, or a coiled up spring, may be carried for any number of miles, and can be made to give out power whenever required at a distant station. All this is true enough and physically feasible, but the whole commercial success or failure of such a scheme must depend, as all commercial schemes must depend, upon its practical utility.

If electrical energy has to be conveyed from one place to another, it is a matter of small commercial importance in the abstract whether it is conveyed by means of metallic conductors or stored up in reservoirs and carried by road or rail; in this the commercial question involved being very much the same as that of the supplying of water by pipes or by water carts. There can be no doubt, however, about which system is, save in exceptional cases, the most convenient, and unless it can be shown that the charging and transmission of storage reservoirs offers advantages upon economical grounds, or very substantial conveniences of application over the system of transmission by conductors, we cannot see that its commercial application upon a large scale can be as remunerative to its proprietors as its promoters would wish to make the public believe.

That M. Reynier should have infused an undue amount of enthusiasm into his paper read before the Société d'Encouragement was natural, considering that he was describing for the first time results far in advance of anything of the same nature than had been achieved

before, and it was perhaps pardonable that he should, for the greater glory of the Faure battery have depreciated the capacity of that of Planté. But in justice to the latter gentleman, and also to arrive at a just appreciation of the real value of the new discovery, careful investigation and comparison between the two have to be instituted. This has been partially done by M. E. Hospitallier since M. Reynier read his paper, and the last number of our excellent conpemporary *L'Electricien* the results obtained are referred to, and the details of the experiments made are to be published in a succeeding number of the same periodical.

Criticising M. Reynier's paper, M. Hospitallier challenges several statements made in that communication. M. Reynier maintained that the Faure battery would give out 80 per cent. of the total power used in charging it. But as M. Hospitallier points out, under the best conditions not more than 90 per cent. of actual work can be transformed from mechanical into electrical energy by a dynamo-electric machine. M. Planté clearly demonstrated that his secondary battery could only give out 88 or 89 per cent. of the power charged into it, and as the difference between it and the Faure battery is one of degree and not of principle, it is not probable that a greater percentage than this could be obtained; possibly it would be less. Finally, a loss of 20 per cent. at least must be allowed for in converting the electrical power in the battery into mechanical force. Making allowances for all these losses it follows that the utmost useful work that can be got from the battery is 52.5 per cent. of the energy employed in charging it, while at the present time it is easy with the ordinary system of conductors to obtain 60 per cent. Passing on to the question of the power which can be stored up in the Faure battery, M. Hospitallier makes an important statement in reply to the assertion of M. Reynier, that this battery can store up forty times as much force as the Planté battery. In conjunction with M. Frank Géraldy, M. Hospitallier has conducted a series of experiments on the Planté battery. The details of these experiments will be published shortly, but the results are given as follows in *L'Electricien:* "Admitting on the one hand as correct the figures given by M. Reynier, that is to say, that a Faure battery, weighing 165 lbs., can give out one horse power during one hour, and on the other hand our experiments on the Planté batteries, the storage power of the Faure batteries varies from one and a half to three times the power of the Planté, according to conditions which we

shall shortly publish. This result is very far from the forty-fold result given by M. Reynier, who obtained it no doubt from imperfect or badly-proportioned Planté batteries, and these results cannot consequently be accepted."—*Engineering.*

Reynier's Constant Battery.—M. Reynier has made a modification of Becquerel's hydro-electric battery, which is comparable to the nitric acid couples in energy without having their inconveniences. The zinc is plunged into a solution of caustic soda; the negative electrode, which is of copper, is depolarized by a solution of sulphate of copper, separated from the alkaline liquid by a permeable cell. The couple thus constituted is constant; its electro-motive force varies from 1·3 to 1·5 volts, according to the concentration of the liquids. The moderate conductibility of the liquids is modified by the addition of salts suitably chosen, and the resistance of the porous cell is reduced by making it of parchment paper, as had previously been done by F. Carré. This battery can be regenerated so readily that the inventor hopes to make it applicable with great economy to small electric motors and to lighting private apartments.—*Comptes Rendus.* C.

Reversion of Photographic Images by Prolonged Illumination.—Janssen has found that photographic images may be inverted and pass from the negative to the positive state by the prolonged action of the light, which produces them. At Meudon the solar images are obtained in a time which varies, according to the state of the atmosphere and the nature of the phenomena, which are to be studied; the time is rarely greater than $\frac{1}{1000}$ of a second when they wish to obtain the photospheric granulations. When the photographic plates are prepared with gelatino-bromide of silver the time may be reduced to less than $\frac{1}{20000}$ of a second. If one of these dry plates receives the light for half a second or a second, the development brings out a positive image, with a white disc and black spots, as the sun appears through a telescope. This positive image may be as well defined as the negative image, which it has replaced. There is an intermediate time between those which give the opposite images, for which time the image is neither positive nor negative, but the plate presents a tint sensibly uniform. Similar inversions may be produced in views of landscapes by giving a sufficiently long exposure.—*Compt. Rend.* C.

Thermo-Dynamics of Liquid Surfaces.—Van der Mensbrugghe is still continuing his study of the application of the second law of thermo-dynamics to the variations of potential energy in liquid surfaces. He is giving special study, by means of some of the most salient facts, to the great cycle of change which embraces the evaporation of the superficial layers of ocean waters, the elevation of vapors into the atmosphere, their condensation into mists and clouds, their fall in rain or snow, the consequent production of glaciers, torrents and rivers, the circulation of water courses, and their return to the bosom of the ocean. He hopes to attract the attention of meteorologists to his investigations in order to induce new and extensive researches in the direction of his inquiries.—*Bull. de l'Acad. Belg.* C.

Intermittent Luminous Signals.—In the ordinary use of lamps for light-house signals the intermittences are produced by a diaphragm which moves before the light, so that the fuel is wasted during the eclipses. At present the average waste of light is about sixty-five per cent., but if a signal was sent twice a minute, sufficient to indicate the first two letters of the light-house, there would be a waste of about ninety per cent. In order to remedy this extravagance Mercadier proposes to adopt a Dubosq lamp with a round wick and a tube in the centre of very small diameter, through which a jet of oxygen can be discharged upon the top of the wick. In spite of the high temperature of combustion, the lamp does not heat much; it consumes little petroleum, and the wick does not crust. Therefore it will operate for many days without being trimmed or filled anew. The intense flame is produced by the combustion of petroleum vapor at the centre of the jet, and the surrounding film of air being a bad conductor the lamp heats only at the top of the burner. The oxygen is enclosed in a reservoir, under suitable pressure, which in his apparatus does not exceed four millimetres (·157 in.) of mercury; it first passes through a manipulator, which has a form similar to that of the key of a Morse instrument, traversing a caoutchouc tube, which is pressed together when the key is at rest. Upon depressing the key the pressure upon the tube ceases, and the oxygen reaches the flame; when the key is released the oxygen jet is stopped. In this manner the flow of oxygen is manipulated as simply as the electric current in the Morse system. The rapidity of manipulation is more than sufficient for all the requirements of optical telegraphy. A method somewhat similar has been contrived by Mercadier for the electric light.—*Compt. Rend.* C.

Remarkable Solar Protuberance.—About 11 A.M. on the 30th of August L. Thollon noticed a small and very brilliant luminous jet near the sun's equator. About 12 h. 45 m. it had attained prodigious proportions, while still preserving the form of a luminous jet from a direction nearly normal to the border of the sun. The result of frequent measurements indicated a height of protuberance at least equal to half of the sun's radius, or more than 200,000 miles. Whilst the lower and middle parts of the protuberance gave a deviation of the C line towards the violet end of the spectrum the summit presented a similar deviation towards the red end.—*Compt. Rend.* C.

Temperature of Least Resistance in Steel.—It is well known that a steel that is very flexible when cold breaks at the blue annealing temperature. It has generally been considered that the purer the iron is the less subject it becomes to this defect, but the workmen of the Ural mountains, who use irons of remarkable purity, have often observed the same fact. Mr. Adamson has found that the metal becomes powdery at a temperature between 260° and 370°C. (500 and 698°F.) or the temperature at which willow twigs take fire. This phenomenon seems to explain a large number of accidents, as for example the breaking of tires under the action of brakes and the fracture of riveted moulds and of machine arbors which become heated by friction.—*Ann. du Gen. Civ.* C.

Behavior of Gas under High Pressures.—The following are some of the results of Amagat's investigations upon the dilatation and compressibility of gases under extreme pressures. 1. The coefficient of dilatation, for temperatures not greatly exceeding the critical temperature, increases with the pressure until it reaches a maximum and then decreases indefinitely. 2. This maximum corresponds to the pressure at which the product $p\,v$ is a minimum. 3. The maximum diminishes with the increase of temperature and finally disappears. 4. At a temperature sufficiently high the compressibility is represented by the formula $p\,(v\,a) =$ a constant; a being the smallest volume that the fluid can occupy; for each gas a has a special value. The values of a for three important gases, at the freezing point and under normal pressure, are as follows: Carbonic acid ·00170, ethylene ·00232, hydrogen ·00078.—*Compt. Rend.* C.

Franklin Institute.

HALL OF THE INSTITUTE, June 15th, 1881.

The stated meeting was called to order at 8 o'clock P.M., the President, Mr. William P. Tatham, in the chair.

There were present 113 members and 59 visitors.

The minutes of the last meeting were read and approved.

The Actuary presented the minutes of the Board of Managers, and announced that at the last meeting 16 persons were elected members of the Institute; also, that upon the recommendation of the Committee on Instruction the Board had decided to create three professorships, viz.: on mechanics, physics, and chemistry; also, that a chemical section had been authorized on the petition of the requisite number of members of the Institute.

On motion of Mr. Washington Jones the regular order of business was suspended, and the Institute proceeded to take a vote upon the repeal of Section 4 of Article III of the By-Laws, and the alteration of Section 1 of Article I, as published in the JOURNAL last month. Both were unanimously adopted, and the confirmation of the latter submitted to a vote of the stockholders present, with a similar result.

The committee on the subject of a reorganization of the Committee on Science and the Arts reported progress.

The following donations to the Library have been received:

Annual Report of the Chief Engineer of the Water Department for 1880. From the Chief Engineer.

Report of the Commissioner of Internal Revenue for year ending June, 1879. From the Commissioner.

Thirtieth Annual Report of the Indiana State Board of Agriculture, 1880. From the Board.

Reports of the Auditor-General on the Finances of the Commonwealth of Pennsylvania for 1878–80.
From Auditor-General, Harrisburg.

Annual Reports of the Secretary of Internal Affairs. Parts 1 and 2, 1879 and 1880. From the Secretary, Harrisburg.

Annual Report of the Secretary of Internal Affairs. Part 4, 1879–80. From the Secretary, Harrisburg.

Annual Report of the Chief Engineer U. S. A. for 1880.
From the Chief of Engineers.

Documents relating to the Colonial History of the State of New Jersey. By W. A. Whitehead. Vol. 1. 1631–87.
From the Author.

Transactions of the Department of Agriculture of Illinois. Vols. 9 and 11. 1871 and 1873. From E. Hiltebrand.

Wood-working Tools; How to Use Them. Boston, 1881.
From the Smithsonian Institute, Washington, D. C.

Transactions of the Society of Engineers for 1880.
From the Society, London.

Specifications and Drawings of Patents for September, 1880.
From the United States Patent Office.

Account of the Operations of the Great Trigonometrical Survey of India. Vol. 6.
From His Excellency the Viceroy and Governor-General of India.

Transactions and Proc. of New Zealand Institute. Vols. 1, 5 to 10, and 13.
Index to Vols. 1 to 8.
Catalogue of Tertiary Mollusca and Echinodermata of New Zealand. Wellington, 1873.
Geological Report on the Waikato District.
Hand Book of New Zealand. Wellington, 1880.
Appendix to Official Catalogue International Exhibition, Sidney. Wellington, 1880.
Palæontology of New Zealand. Part 4. Wellington, 1880.
Fifteenth Annual Report on the Colonial Museum, etc., 1879–80.
Catalogue of Exhibits International Exhibition. Melbourne, 1880.
Reports of Geological Explorations during 1877–79.
Catalogue of Stalk and Sessile-eyed Crustacea of New Zealand.
Catalogue of the Echinodermata of New Zealand.
From the New Zealand Institute, Wellington, N. Z.

General Index to the Fourth Ten Vols. of Jour. of Royal Geographical Society. 1881. From the Society.

Memoirs of the Literary and Philosophical Society of Manchester. Vol. 6. 3d Series.
Proceedings of the Literary and Philosophical Society of Manchester. Vols. 16–19. From the Society.

Official Catalogue of the British Section Paris Univ. Exhibition, 1878. 2d Ed. 4 Pts.

Remarks on the Manufacture of Glucose by the Johnson Process.
From Harrison Bros. & Co.

Photometric Measurements of the Variable Stars. By E. C. Pickering. Cambridge, 1881, From the Author.

Hygiene of Emigrant Ships. By T. J. Turner. 1880.

Fourteenth Annual Report of the Peabody Institute of the City of Baltimore. 1881. From the Institute.

Catalogues des Brevets, 1878–80.
From the French Patent Office.

Report upon Certain Museums for Technology, Science and Arts. By A. Liversidge. Sidney, 1880.

Reports of Council of Education upon the Condition of Public Schools, 1879. Sidney, 1880.

Journal of the Royal Society of New South Wales. Vol. 13. 1879.

Transactions of the Royal Society of New South Wales for 1868–70 and 1873.

Annual Report of the Department of Mines, with Maps of New South Wales, for 1878 and 1879.
From the Royal Society of New South Wales.

Abhandlungen der K. K. Geol. Reich. Vol. 12. Part 2. Die Gasteropoden. From K. K. Geol. Reich., Vienna.

Programm der Grossherzoglich-Badischen Polytechnischen Schule zu Karlsruhe für 1880–81. From the School.

Publications of Royal Instituto di studi superiori practici e di perfezionamento in Firenze.

(1) Colera Asiatica memoire del Dott Filippo Pacini (2) Origini Della Lingua Poetica Italiana Del Dott C. N. Caix. From the Institute.

Meteorological Observations recorded at Six Stations in India in 1879.

Registers or Original Observations in 1880 for January to March.
From the Meteorol. Dept. Government of India.

Anales del Instituto y Observatorio de Marina de San Fernando. By Don Cecilio Pujazon. Section 2. 1877 and 1878.
From the Institute.

Dr. Robert Grimshaw read a paper on "Percussion Rock Drills," illustrated by photographs of machines projected upon the screen. The paper is printed in this number of the JOURNAL.

Mr. Lorin Blodgett read a paper on "Textile Fibres Under the Microscope," illustrated by samples of them, and photographs of the same thrown upon the screen; also images of the silkworms at work. The cocoons, etc., were furnished by the Women's Silk Culture Association. Mr. Blodgett, after briefly mentioning the importance of the subject upon which he had undertaken to speak, said that he did not purpose to repeat the admirable general directions to silk-growers given by the Women's Association, but simply to show what the silk fibre

is, and how to handle it, so as to make it as valuable as the silks of Italy or France.

The fibre of silk as produced in the cocoon is a single, continuous and perfect fibre—the most perfect and durable of fibres, if properly treated; and it must be treated as a single fibre throughout. *Raw silk* is a definite number, five to eight fibres, as reeled from the cocoons, adhering in a body by the gum of the cocoon remaining on them—apparently a single fibre, but really a bundle of eight, as shown in the figure, representing a thread of Chinese *Tsatlees*, the best of the Chinese raw silks.

All silk fibres must be reeled in bundles of not less than five nor more than eight; if reeled in single, double or even triple fibres, from as many cocoons, they cannot be used as regular silk.

Floss silk is of two kinds; first, the light outer fibres of the cocoon, with the broken and imperfect fibres cleaned of gum, usually; but the better *floss silk* is reeled silk of not less than five cocoon fibres, not usually cleaned of gum, and not much twisted. This is more frequently called *singles*.

Tram is a combination of three threads, 15 to 24 original fibres, with more twist, $2\frac{1}{2}$ to 3 turns to the inch.

Organzine is made up of two threads, twisted 12 turns per inch to the left, then doubled with 8 turns per inch to the right. This is the standard quality of thread for the best silk goods.

All who reel silk should weigh 500 yards of the raw thread, made up of five single cocoon fibres, making 2500 yards of single fibre. If the hank so reeled is above or below the standard weight, a greater or less number of cocoons should be united in the one raw thread.

These definitions are given because they are absolutely essential to success on the part of the American grower. He cannot make anything but raw silk in the gum, as reeled in the manner described; but he must know what the manufacturer who makes floss, tram and organzine demands, or his silk will be worth but one or two dollars per pound, when it should be worth six dollars per pound.

Spun silk is made from pierced cocoons, cocoon waste, and the waste mills using raw silk—that is, in its best form. It is carded and drawn with as much care as worsted wool, and forms a valuable element of many fabrics, particularly upholstery goods, trimmings and ornamental articles. All growers will, of course, have a part of their

product in these waste forms, but they alone do not make silk growing profitable.

The lecturer also described briefly other textile fibres, such as wool, cotton, jute, flax, ramie, etc., and showed on the screen magnified images of dyed silk with particles of iron appearing like knots on the fibre. These add weight to the goods, but reduce its wearing qualities and value. They are used to a great extent in some silks, the pound of silk sent to the dyer coming back weighing from 17 to 18 ounces, the additional weight being due to iron, tin or chemicals used in dyeing.

Mr. Orr said that he thought that flax had been ascertained to have a definite fibre of known diameter. It was an old homely fibre, but very good, and he was glad to say that the State of New Jersey had offered a premium of $5.00 per ton for flax straw grown in that State, in order to encourage that important industry.

The Secretary's report included the Servoss Gas Regulator, which was shown and explained. It contains no diaphragms of rubber or leather, liable to become clogged and stiff, but is made entirely of brass. It is attached to the discharge side of the meter, the house pipes being then screwed to the regulator instead of to the meter. The pressure of gas in the street mains is greater, in parts of a district at least, than is required to furnish full light without waste, this being made necessary by the length of the mains and the variable number of jets in use at different times. The regulator is intended to reduce the pressure in the pipes to which it is attached, so that no more gas shall pass than can be economically burned. In the Servoss Regulator there are two brass valves, one of which can be fixed and locked to limit the passage of gas for a fixed number of lights, while the other moves automatically and accommodates itself to the number of burners in use, the weight of this valve being gauged to the average gas pressure in the place at which it is to be employed. It has been in use for five years, and the patentee claims that it will save 15 per cent. of gas in this city without decreasing the illumination.

Amesbury's band saw filing machine was exhibited. It is designed not only to expedite the work of sharpening band saws, but to secure the even cutting of the points. It is said that an ordinary band saw contains from 500 to 1800 teeth, and that it takes an expert filer from 30 to 90 minutes to sharpen one, while a boy, who simply turns a crank, can with the machine do the same work better in from 10 to 15 minutes. Two special files are used, one for sharpening the face of